Working Memory Capacity and the Top-Down Control of Visual Search: Exploring the Boundaries of "Executive Attention"

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Abstract:

The *executive attention theory* of working memory capacity (WMC) proposes that measures of WMC broadly predict higher order cognitive abilities because they tap important and general attention capabilities (R. W. Engle & M. J. Kane, 2004). Previous research demonstrated WMC-related differences in attention tasks that required restraint of habitual responses or constraint of conscious focus. To further specify the executive attention construct, the present experiments sought boundary conditions of the WMC–attention relation. Three experiments correlated individual differences in WMC, as measured by *complex span* tasks, and executive control of visual search. In feature-absence search, conjunction search, and spatial configuration search, WMC was unrelated to search slopes, although they were large and reliably measured. Even in a search task designed to require the volitional movement of attention (J. M. Wolfe, G. A. Alvarez, & T. S. Horowitz, 2000), WMC was irrelevant to performance. Thus, WMC is not associated with all demanding or controlled attention processes, which poses problems for some general theories of WMC.

Keywords: working memory, executive control, visual search, individual differences, attention

Article:

Individual differences in working memory capacity (WMC), as measured by tasks such as reading span (Daneman & Carpenter, 1980), strongly predict individual differences in a wide range of fluid cognitive capabilities, including language comprehension, learning, and reasoning (for reviews, see Conway, Kane, & Engle, 2003; Daneman & Merikle, 1996; Engle, Kane, & Tuholski, 1999). However, because WMC tasks are themselves complex, the cognitive processes that drive these empirical associations are not transparent. Consider the reading span task, which typically presents short lists of words to remember, with each memory item preceded by the presentation of an unrelated comprehension task, such as reading or evaluating sentences. Subjects must effectively encode, maintain access to, and/or recover the current set of target words in the face of interruption and disruption from the reading task and proactive interference from prior sets. Thus, reading span scores may predict other cognitive abilities because they reflect reading skill (Daneman & Carpenter, 1980, 1983), storage capacity or memory decay (Bayliss, Jarrold, Gunn, & Baddeley, 2003; Towse, Hitch, & Hutton, 2000), mental resources or activation (Cantor & Engle, 1993; Just & Carpenter, 1992; Shah & Miyake, 1996), processing efficiency or speed (Bayliss et al., 2003; Case, Kurland, & Goldberg, 1982; Fry & Hale, 1996), mnemonic strategies (McNamara & Scott, 2001), inhibitory control of memory interference (Lustig, May, & Hasher, 2001), a limited structural capacity for focused attention or mental binding (Cowan, 2005; Oberauer, 2005), or a combination of these mechanisms. Although some of these explanations for WMC tasks' predictive power seem to fail critical tests (see Engle & Kane, 2004; Engle, Kane, et al., 1999), no consensus view has yet emerged from this active research area (e.g., Miyake, 2001; see also Ackerman, Beier, & Boyle, 2005; Beier & Ackerman, 2005; Kane, Hambrick, & Conway, 2005; Oberauer, Schulze, Wilhelm, & Süß, 2005).

Our own view, which motivated the present set of experiments, is that WMC tasks reflect a host of cognitive processes and abilities, but the ones that are primarily responsible for the relation between WMC and general fluid abilities (Gf) are *attentional*. This *executive attention* view argues that WMC tasks predict individual

differences in Gf because they reflect, in part, the controlled, attentional, and domain-general ability to maintain or recover access to stimulus or goal representations outside of conscious focus.[1] This ability is most important and most easily measurable when people must keep representations accessible in the face of proactive interference or habits from prior experiences and in the face of distraction from other mental or environmental events (Engle & Kane, 2004; Engle, Tuholski, Laughlin, & Conway, 1999; Heitz, Unsworth, & Engle, 2005; Kane & Engle, 2002; for related views, see Hasher & Zacks, 1988; Hasher, Zacks, & May, 1999; Lustig et al., 2001).

To summarize the supporting evidence (see Engle & Kane, 2004, for a more detailed review), we note that WMC tasks correlate with Gf measures more strongly than do "simple" short-term memory (STM) span tasks that require only immediate recall of lists. Indeed, a recent review suggested that WMC accounts for about half the variability in Gf among healthy adults (Kane et al., 2005). Moreover, residual variance from WMC tasks continues to predict Gf after STM variance, which may be interpreted as nonattentional storage variance, is partialed out (Bayliss et al., 2003; Conway, Cowan, Bunting, Therriault, & Minkoff, 2002; Engle, Tuholski et al., 1999; Oberauer et al., 2005; but see Colom, Rebollo, Abad, & Shih, 2006). More direct support comes from extreme-group-design studies contrasting top-quartile scorers on WMC span tasks (*high spans*) with bottom-quartile scorers (*low spans*) in a variety of memory- and attention-control tasks. For example, in memory interference tasks, high spans show less vulnerability to competition at retrieval than do low spans (e.g., Bunting, 2006; Bunting, Conway, & Heitz, 2004; Conway & Engle, 1994; Lustig et al., 2001; Rosen & Engle, 1998). Many theories suggest that such effective interference resistance is diagnostic of effective attention control (e.g., Anderson, 2003; Dempster, 1992; Hasher & Zacks, 1988; Norman & Shallice, 1986), and, indeed, experimenters can make high spans as vulnerable to interference as low spans by dividing their attention during the memory task (Kane & Engle, 2000; Rosen & Engle, 1997).

Even in more simple tests of attention control that do not heavily tax memory, high spans outperform low spans. This is particularly true when the attention task requires novel goals to be accessibly maintained and habitual responses to be withheld, such as in the antisaccade task, which requires subjects to move their eyes and attention in opposition to a salient visual stimulus (Kane et al., 2001; Unsworth, Schrock, & Engle, 2004), and in the Stroop task, in which subjects must respond to the hue rather than the identity of color words (Kane & Engle, 2003; Kiefer, Ahlegian, & Spitzer, 2005; Long & Prat, 2002; McCabe, Robertson, & Smith, 2005). As well, high spans' responses to target stimuli are less disrupted by the presentation of distractors than are low spans' responses in dichotic listening and visual flanker tasks (Conway, Cowan, & Bunting, 2001; Heitz & Engle, 2006; Reddick & Engle, in press; but see Friedman & Miyake, 2004). Intervention studies provide further experimental evidence: Providing healthy people and attention-deficit/hyperactivity disorder patients with extensive training on WMC tasks significantly improves their scores on attention control and fluid-ability tasks, such as Stroop and progressive matrices (e.g., Klingberg et al., 2005; Klingberg, Forssberg, & Westerberg, 2002).

Despite considerable evidence in favor of the executive attention view of WMC and its relation to complex cognition, significant challenges remain (see Friedman & Miyake, 2004; Kane et al., 2005). First, but not addressed in the present work, is that we do not yet know that the variance shared between WMC and Gf tasks is the same as that shared between WMC and attention control tasks. These associations have almost always been tested in separate studies, so it remains possible that different cognitive processes mediate them. Indeed, as they are typically measured, WMC may correlate more strongly with Gf than does attention control (e.g., Friedman et al., 2006; Schweizer & Moosbrugger, 2005), which would suggest that aspects of WMC beyond attention may contribute to Gf correlations. Large-scale latent variable studies using WMC, attention control, and Gf tasks are required, therefore, to settle the issue of whether executive attention processes are critical to WMC's predictive power. Second, and at the focus of the present experiments, is that the executive attention construct is inadequately specified. Like the concepts of controlled processing, executive function, top-down control, attention, and obscenity, one may know it when one sees it, and researchers may be able to create a consensus list of its attributes (e.g., Hasher & Zacks, 1979; Norman & Shallice, 1986; Posner & Snyder, 1975;

Shiffrin, Dumais, & Schneider, 198 1), but scientific progress eventually demands stricter operationalization than we have yet offered.

As we have noted, we characterize executive attention as comprising those domain-general processes that keep stimulus and goal representations accessible outside of conscious focus, which are most useful and detectable under conditions of interference, distraction, and response competition (Engle & Kane, 2004). How-ever, if executive attention processes are as general as we have proposed (and as some views of executive function suggest; e.g., Norman & Shallice, 1986), then they ought to contribute to some aspects of cognitive control beyond those tapped by interference or conflict tasks, such as Stroop, antisaccade, and dichotic listening. After all, the higher order abilities that WMC predicts do not all seem to involve much competition or conflict.

Indeed, there is accumulating evidence that individual differences in WMC also predict variation in performance of "controlled" visual attention tasks in which prepotent response tendencies play a less obvious role. For example, Conway, Tuholski, Shisler, and Engle (1999) found that high-WMC-span subjects showed larger negative priming effects in a letter-naming task than did low spans, and Engle, Conway, Tuholski, and Shisler (1995) found that putting subjects under a simultaneous working memory load decreased their negative priming effects. Although the under-lying causes of negative priming are still under debate (e.g., Houghton & Tipper, 1994; Milliken, Joordens, Merikle, & Seiffert, 1998; Neill & Valdes, 1992), these tasks required subjects to selectively attend to one visual stimulus while ignoring another, without any strong habitual tendency to attend to distractors. Similarly, in a response-deadline version of the Eriksen flanker task (Eriksen & Eriksen, 1974) that presented a target letter surrounded by four distractor letters, high spans reached asymptotic accuracy at much shorter deadlines than did low spans (Heitz & Engle, 2006; see also Reddick & Engle, in press). Because both groups eventually reached identical asymptotes, Heitz and Engle argued that high spans demonstrated effective control by more rapidly restricting attentional focus than did low spans, from its originally diffuse mode, encompassing the entire array, to a tighter lock onto the target (see Gratton, Coles, & Donchin, 1992; Gratton, Coles, Sirevaag, Eriksen, & Donchin, 1988).

WMC-related differences in controlling attentional focus, again in the absence of strong prepotencies, were perhaps most dramatically demonstrated by Bleckley, Durso, Crutchfield, Engle, and Khanna (2003). Subjects identified a centrally presented letter at the same time they localized another letter to 1 of 24 locations along three concentric rings around fixation (see Egly & Homa, 1984). All displays were masked after very brief exposures that were individually tailored to each subject, and the location of the peripheral stimulus was endogenously cued in advance of each trial by the word *inner*, *middle*, or *distant*. Cues were valid for 80% of the trials, so attending to them generally improved performance. However, on the critical, invalidly cued trials that presented a target on a ring interior to the cued ring (e.g., in the inner ring when the middle one was cued), low spans actually localized targets more accurately than did high spans, and they did so as accurately as on valid trials. These findings suggest that low spans focused their attention like a spotlight, highlighting the cued ring and everything within it. In contrast, high spans appeared to more effectively limit their visual attention to the cued ring. When a target appeared interior to the cued ring, high spans localized it no more accurately than they did those that appeared outside the cued ring—even though the target was closer to fixation than was the cued ring. Bleckley et al. (2003) argued that executive attention is necessary to build and maintain endogenously cued object representations. As further support for this idea, Bleckley and Engle (2006) found that high spans performed like low spans, showing spotlightlike focus, under dual-task conditions that stressed their executive control processes. Moreover, low spans performed like high spans, showing object-based focus, only when the target ring was exogenously cued by its flashing, thus relieving executive processes from duty.

Together, then, the findings from attention tasks involving response conflict and endogenous focusing—that is, those requiring either the *restraint* of habitual behavior or the *constraint* of visual or auditory attention—suggest that WMC correlates broadly with diverse indicators of top-down executive control. Thus, WMC does not seem to be uniquely associated with any particular control function. The present experiments further test the boundaries of the WMC–executive attention construct by examining the performance of high- and low-span subjects in a variety of visual search tasks. Although visual search is rarely mentioned in the context of

executive functions (see Monsell, 1996) and is not strongly associated with deficits accompanying prefrontal cortex damage, the same can be said for flanker-type tasks (e.g., Lee, Wild, Hollnagel, & Grafman, 1999; Rafal et al., 1996), which do produce WMC-related differences in performance. Moreover, suggestive evidence of a WMC–search association comes from an enumeration study in which subjects counted between 1 and 12 visual targets on each trial (Tuholski, Engle, & Baylis, 2001). High spans responded more quickly than low spans amid increasing numbers of conjunctive distractors, which shared features with the targets; the span groups were equivalent, however, with increasing numbers of disjunctive distractors, which shared no features with targets. These findings are clearly reminiscent of classic visual search results, in which increasing numbers of conjunctive distractors in a display led to steeper search response time (RT) slopes than did increasing numbers of disjunctive distractors (e.g., Treisman & Gelade, 1980).

Indeed, several theories of visual search, particularly those arguing for self-terminating serial search under some contexts, propose an important role for attention and top-down control processes. For example, both feature integration theory (Treisman & Gelade, 1980; Treisman & Gormican, 1988; Treisman & Sato, 1990) and guided search theory (Cave & Wolfe, 1990; Wolfe, 1994; Wolfe, Cave, & Franzel, 1989) propose that some primitive features of visual scenes are analyzed and represented in parallel across the visual field but that identification and localization of objects usually requires attention to be serially deployed to bind together the features at potential target locations.[2] These models also include a role for subjects' top-down control over the coordination of attention. In guided search, attention is guided across locations on the basis of activation levels in a master map that receives both bottom-up and top-down input. Bottom-up activation from independent feature maps (e.g., representing color or line orientation) is based on local feature distinctiveness, and top-down activation derives from coarse categorical knowledge about the target's features (e.g., that it is likely to be red or horizontally oriented). Top-down control may operate to increase the activation of a feature that is likely to identify the target (e.g., to all locations of red things), to decrease the bottom-up activation from a feature map that is associated with distractors (e.g., if targets are red horizontal lines and there are green horizontal distractors, bottom-up input from orientation would be reduced), or both.

Regardless of how such top-down input might be implemented (feature integration theory emphasizes inhibition, whereas guided search theory emphasizes activation), these theories propose some endogenous control over the deployment of attention in visual search, suggesting that it may be a profitable testing ground for attentional theories of WMC. Indeed, there are myriad findings indicating that subjects can sometimes exert strategic control over search (e.g., Bacon & Egeth, 1997; Duncan, 1989; Egeth, Virzi, & Garbart, 1984; Green & Anderson, 1956; Moore & Egeth, 1998; Rabbitt, 1984; Wolfe et al., 1990; Zohary & Hochstein, 1989) and that individual differences in search are robust (Wolfe, Friedman-Hill, Stewart, & O'Connell, 1992). Moreover, related theoretical approaches to search, such as *biased competition* views, propose that an attentional template is held in active memory to control competition from distractors for limited processing capacity (e.g., Bundeson, 1990; Desimone, 1996; Desimone & Duncan, 1995; Downing, 2000; Duncan, 1998; Duncan & Humphreys, 1989). The analogy seems strong to executive attention processes of working memory that maintain access to stimulus and goal representations in the face of competition and conflict (Engle & Kane, 2004; Kane & Engle, 2003).

Thus, if executive attention is involved in many (or most) varieties of attention control, even in those that do not involve the restraint of habitual responses, the constraint of visual focus, or the resolution of interference, then WMC-related individual differences should emerge in search performance. If, however, the attention processes involved in WMC are more limited in scope, involved in goal maintenance only in the service of controlling conflict and restricting focus, we may observe null effects of WMC.

Rest assured that we recognize the potential circularity in this line of pursuit. We propose to infer executive involvement in tasks by their sensitivity to WMC-related differences, but at the same time we seek to explain those WMC differences by appealing to executive attention. However, in the present experiments we attempt to break this tautology by investigating particular task conditions that other research suggests should be especially likely to evoke endogenous control or to be susceptible to attentional limitations. But, even without this

protective measure, we suggest that our approach has considerable heuristic value. WMC is clearly related to some varieties of attention control (memory-interference tasks, Strooplike conflict tasks, selective-focusing tasks), but to better specify what we mean by an executive attention construct, we must delineate its boundaries by trying to find ostensible attention tasks that are insensitive to WMC differences. An empirical failure to link WMC to visual search would therefore be important to working memory theory. In addition, students of visual search who are interested in the nature of top-down control should gain some theoretical purchase from a finding either that visual search is related to WMC in similar ways as are other controlled tasks or that it is unrelated to WMC and is likely to be controlled via different mechanisms than are those other demanding "attention" tasks.

EXPERIMENT 1

Experiment 1 was motivated by a small pilot study that yielded no evidence for WMC-related differences in visual search. In that study, subjects identified as having high or low WMC, on the basis of their working memory span performance, searched matrices of letters for target *F*s among either *O*s or *E*s in one trial block and for target *P*s among either *C*s or *R*s in another block. We had predicted span equivalence in the efficient pop-out searches for *F*s and *P*s among *O*s and *C*s and had predicted span differences in the inefficient searches among *E*s and *R*s (we expected these latter searches to be inefficient, with increasing RTs with increasing set sizes, on the basis of prior findings from feature-absent searches; Treisman & Gormican, 1988; Treisman & Souther, 1985). As we expected, both high and low spans showed very shallow—and equivalent—search slopes when the targets and distractors were perceptually dissimilar. The span groups were also equivalent, however, in the feature-absent conditions, despite surprisingly steep search slopes overall (approximately 24 ms/item on target-present trials and 55 ms/item on target-absent trials).

In Experiment 1 we attempt to replicate these null pilot findings in a larger scale study using a similar search task. Although, as we have mentioned, search-task methods can be manipulated in ways that elicit controlled, strategic behavior in subjects (e.g., Bacon & Egeth, 1997; Wolfe, Alvarez, & Horowitz, 2000), we wanted to begin our investigation of the WMC–search relation with as simple a method as possible that could still be expected to elicit serial or attention-demanding search and to yield substantial search slopes. Recall that our goal is to push the boundaries of the executive attention construct, and this requires investigating search tasks with little in common with attention-control tasks that have repeatedly been associated with WMC differences. In this experiment, then, high- and low-WMC subjects searched visual displays for a target *F* among either *O*s (for efficient search) or *E*s (for inefficient search). To test whether our prior null findings arose because we presented stimuli in regular, 4 X 4 matrix displays, we also manipulated the degree of organization of search displays between trial blocks.

Method

WMC Screening

We individually screened approximately 500 undergraduates (ages 18–35 years) from the University of North Carolina at Greensboro and Southern Illinois University at Edwardsville for WMC using the Operation Span (OSPAN) task (Turner & Engle, 1989). Each trial presented a series of two to five simple mathematical operations to solve, interpolated with two to five unrelated words to memorize. An E-Prime 1.0 program (Schneider, Eschman, & Zuccolotto, 2002) presented the stimuli, in black against a white background, at the center of a color monitor. This version of OSPAN is highly reliable, demonstrating good internal consistency and strong correlations with other putative WMC tasks (see Kane et al., 2004).

One operation–word string appeared at a time, and subjects read the operation out loud—for example, "Is (9/3) + 2 = 5?"—verified whether the provided answer was correct (i.e., "yes"), and then immediately read the word aloud (e.g., "drill"). The experimenter then immediately pressed a key to advance the screen either to the next operation–word string or, if the trial was complete, to the recall cue (a set of three centered question marks). If an operation–word string appeared, subjects read it aloud with-out pausing. If the recall cue appeared, subjects used an answer sheet to report all the words from the trial in serial order.

Three trials were presented at each length (two through five), in the same pseudorandom sequence for all subjects. The OSPAN score was calculated as the sum of recalled words from all trials that were completely recalled in correct serial order (maximum score = 42).

Visual Search

Subjects

One hundred twenty of the screened subjects participated in Experiment 1 as partial fulfillment of a course requirement. Fifty-six subjects were selected as having OSPAN scores from the top quartile of our typical distribution (i.e., at least 19; high spans), and 64 were selected from the bottom quartile (i.e., 9 or lower; low spans). All subjects who participated in the visual search experiment had responded correctly to 85% or more of the OSPAN operations, and they completed the visual search experiment within the same academic semester as the OSPAN screening.

Design

The design was a 2 X 2 X 2 X 3 X 2 mixed-model factorial, with working memory (WM) span (high, low) as a between-subjects variable; array type (more organized, less organized) manipulated within subjects and between trial blocks; and distractor type (*E*s, *O*s), array size (1, 4, 16), and trial type (target present, target absent) manipulated within subjects and within trial blocks. We randomly assigned subjects to one of two array-type order conditions: more organized block first versus less organized block first.

Apparatus and Materials

An E-Prime 1.0 program, running on Pentium III or higher PCs, presented the stimuli on a 17-in. (43-cm) color monitor and collected response latency and accuracy data. We drew the letter search stimuli in Microsoft Paint; all lines were drawn 1 mm (2 pixels) thick. Target *F*s were 7 mm high, with a top horizontal line of 4.5 mm and a middle horizontal line of 4 mm. Distractor *E*s differed from *F*s in that their top and bottom horizontal lines were 5 mm long and their middle horizontal line was 4.5 mm long. Distractor *O*s were 7 mm high and 6.5 mm wide.

Search stimuli appeared in either more organized or less organized arrays (see Figure 1). More organized arrays presented the search stimuli within 16 possible locations, arranged into a 4 X 4 matrix (71 mm high X 70 mm wide). The distances between stimuli in the matrix varied slightly from display to display, depending on the particular composition of letters, and ranged from 14.5 mm to 18 mm apart horizontally and from 13 mm to 15.5 mm apart vertically. Less organized arrays presented the target (*F*) and distractor (*O* or *E*) stimuli in any of the 16 locations from the more organized arrays but also presented distractors in any of the horizontal and vertical spaces between the locations from the more organized arrays. Thus, the minimum possible distance between stimuli was much smaller here than in more organized arrays, ranging from 3.5 mm to 6 mm horizontally and from 2.5 mm to 4 mm vertically.

Subjects saw 192 trials in both the more and the less organized array blocks. In each block, 96 trials presented a target *F* (target-present trials), and 96 trials did not (target-absent trials). Half the trials for each trial type presented *O*s as distractor stimuli, and half presented *E*s, and within each of these distractor types, 16 trials presented 1, 4, and 16 total stimuli for search (for target-absent trials, a distractor stimulus was presented in the place of the target).

On more organized array trials, target and distractor stimuli were presented only in the 16 locations defining a 4 X 4 matrix, not in the spaces between these locations. Targets appeared equally often in each of the 16 locations within each experimental condition. Thus, Array Size 1 trials presented a target (or single distractor) one time in each location. Array Size 4 trials did so, too, and distractors were presented only within the quadrant of the matrix that also contained the target. So, for example, if the target was presented in the topmost left location, a distractor appeared in the location to the right, the location below, and the location diagonally down and to the right. Array Size 16 trials presented distractors in all the locations not occupied by the target (15 locations on target-present trials, and all 16 locations on target-absent trials).

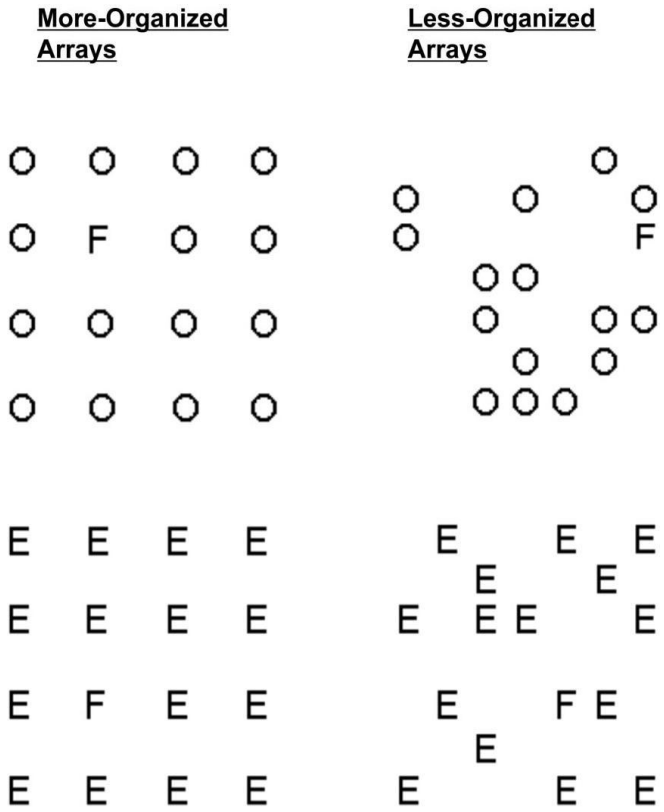


Figure 1. ***Examples of more organized and less organized stimulus arrays from Experiment 1. Subjects searched these arrays for the presence of a target F.***

On less organized array trials, targets appeared in one of the same 16 locations defined by the more organized arrays, and on target-absent trials of array size 1, the single distractor always appeared in 1 of these 16 locations. However, in trials with array sizes 4 or 16, distractors could appear in empty target locations and in empty locations between target locations (horizontally and vertically, for a total of 49 possible locations). Array Size 4 trials randomly presented distractors within the quadrant of 9 locations defined by the target; Array Size 16 trials randomly presented distractors in any of the 49 locations.

All subjects saw the same set of 192 stimulus arrays for more organized blocks and 192 arrays for less organized array blocks, but in a different random sequence. Thirty arrays were created for practice trials in the more organized condition that approximately balanced the experimental conditions of trial type, distractor type, and set size; 24 such arrays were created for less organized practice.

Procedure

We tested all subjects individually. They had to report, as quickly and accurately as possible, whether an *F* appeared in each display via a keypress (the *z* key for *yes* and the slash key for *no*). The task began with a practice block for either the more organized array condition or the less organized array condition, followed by the experimental block. Then a practice block for the other array condition preceded its experimental block. Each practice and experimental trial presented a blank screen for 490 ms, then an asterisk at central fixation for 740 ms, and then the centered stimulus array, which remained on screen until the subject responded. Subjects received no feedback.

Results

Alpha levels were set at .05 for all analyses, and effect sizes are reported as partial eta squared (η^2_P) Cohen (1988) suggested effect sizes of .01, .06, and .14 to indicate small, medium, and large effects, respectively (although such criteria are necessarily arbitrary and perhaps too liberal; Olejnik & Algina, 2000).

Subjects

Data from 3 subjects were dropped because of excessive error rates (at least 50% in any of the experimental conditions), which left 55 high spans and 62 low spans in the analyses.

RTs

Target-Present Trials

Figures 2A and 2B present the means of high- and low-span subjects' median RTs for correct target-present trials, in more organized and less organized arrays, respectively. These two array types elicited similar result patterns, so we analyzed them together in a 2 (WM span) X 2 (array type) X 2 (distractor type) X 3 (array size) mixed-model analysis of variance (ANOVA), with array type, distractor type, and array size as repeated-measures variables.

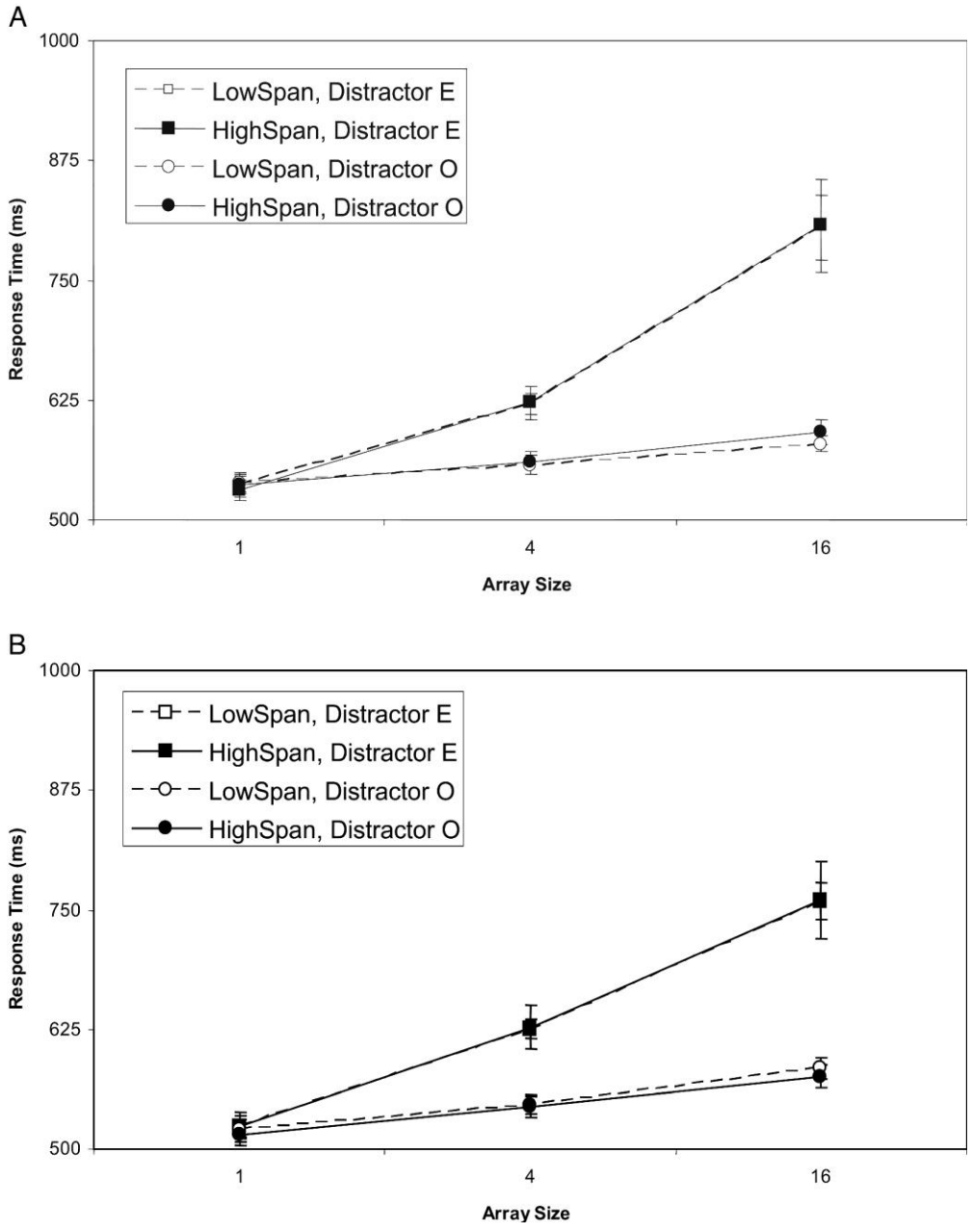


Figure 2. ***Mean target-present response times (in milliseconds) from Experiment 1, by working memory span group (high span vs. low span) and array size, for trials with O distractors versus E distractors. A: Data from more organized arrays. B: Data from less organized arrays. Error bars represent standard errors of the means.***

As expected, subjects located target *F*s significantly more slowly amid distractor *E*s than amid distractor *O*s, $F(1, 115) = 165.93$, $\eta^2_P = .59$, and in larger than in smaller arrays, $F(2, 230) = 216.34$, $\eta^2_P = .65$; searches also slowed more with increasing array size amid distractor *E*s than amid *O*s, $F(2, 230) = 102.37$, $\eta^2_P = 47$. However, despite these large experimental effects, WM span showed none, with only one $F > 1$; for the WM Span X Array Type X Distractor Type interaction, $F(1, 115) = 1.07$, $p = .31$. There was no evidence for a relation between WMC and visual search times. Remaining significant effects were a main effect of array type, $F(1, 115) = 7.06$, $\eta^2_P = .06$; and Array Type X Set Size, $F(2, 230) = 4.38$, $\eta^2_P = .04$; and Array Type X Distractor Type X Array Size interactions, $F(2, 230) = 10.22$, $\eta^2_P = .08$.

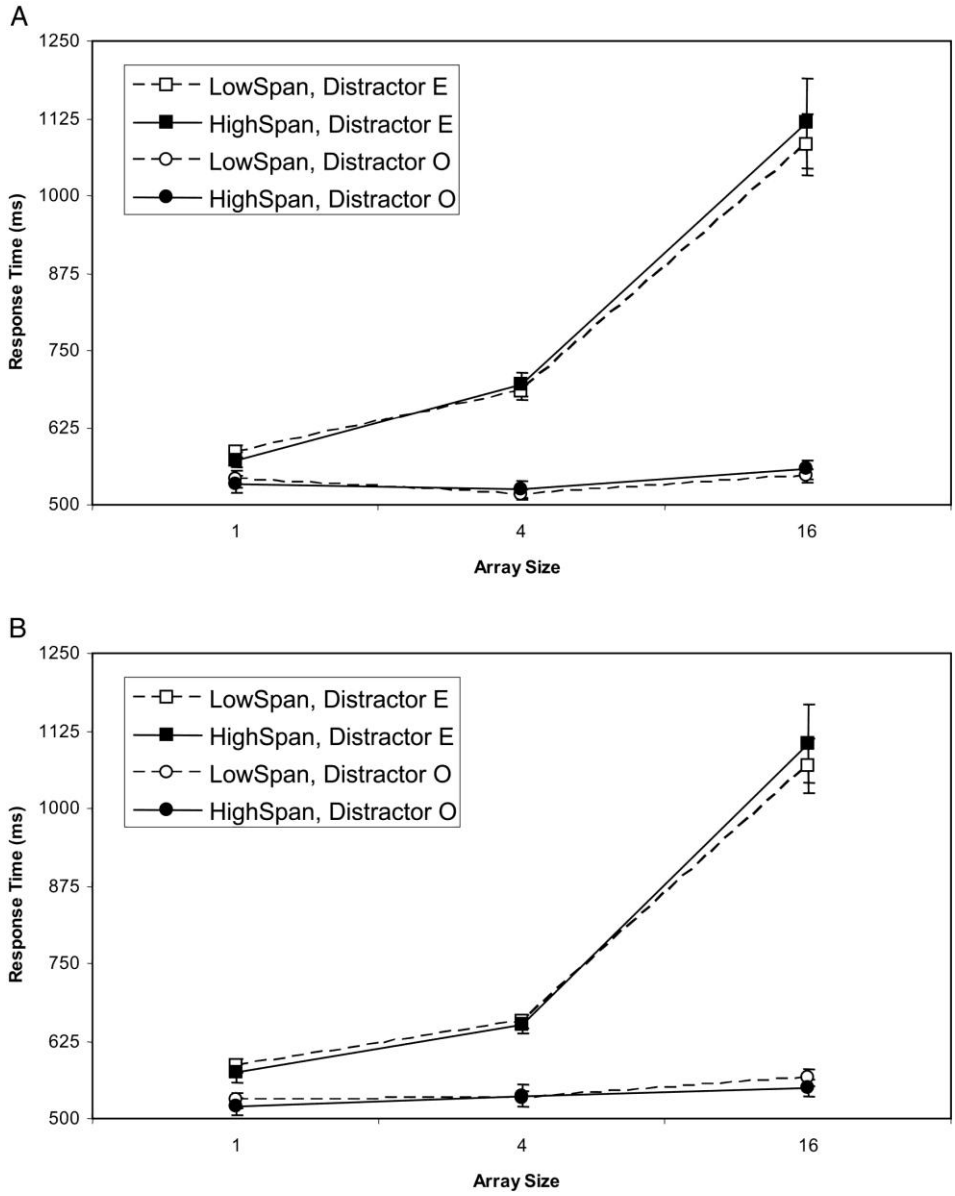


Figure 3. ***Mean target-absent response times (in milliseconds) from Experiment 1, by working memory span group (high span vs. low span) and array size, for trials with O distractors versus E distractors. A: Data from more organized arrays. B: Data from less organized arrays. Error bars represent standard errors of the means.***

Target-Absent Trials

Figures 3A and 3B present the means of high and low spans' median RTs for correct target-absent trials, in more and less organized arrays, respectively. Again, subjects responded more slowly to arrays presenting distractor *E*s than to those presenting *O*s, $F(1, 115) = 391.95$, $\eta^2_P = .77$, and to larger than to smaller arrays, *F*(2,

230) = 193.67, η^2_P = .63; searches also slowed more with increasing array size amid distractor *E*s than amid *O*s, *F*(2, 230) = 187.39, η^2_P = 62. Again, WM span had no significant effects (all *F*s < 1), so we find no evidence for a WMC–search association. Remaining significant interactions were Array Type X Distractor Type, *F*(1, 115) = 4.92, η^2_P = .04, and Array Type X Distractor Type X Array Size, *F*(2, 230) = 4.36, η^2_P = .04.

Error Rates

Target-Present Trials

Table 1 presents the means of high- and low-span subjects' error rates for target-present and target-absent trials, which we analyzed in a 2 (WM span) X 2 (array type) X 2 (distractor type) X 3 (array size) mixed-model ANOVA, with array type, distractor type, and array size as repeated-measures variables. Error rates were very low overall (*M*s < 5% in all conditions). Nonetheless, subjects missed more targets amid distractor *E*s than amid *O*s, *F*(1, 115) = 12.80, n2p = .10, and amid larger than amid smaller arrays, *F*(2, 230) = 7.12, η^2_P = .06; subjects also missed more targets with increasing array size amid distractor *E*s than amid *O*s, *F*(2, 230) = 6.56, η^2_P = .05.

Table 1
Mean Search Error Rates For Experiment 1

Target presence, distractor type, and working memory span	Array size: 1 *M*	1 *SD*	4 *M*	4 *SD*	16 *M*	16 *SD*
More organized arrays						
Target present						
Distractor *O*						
High span	0.015	0.040	0.013	0.033	0.014	0.031
Low span	0.023	0.036	0.021	0.041	0.012	0.027
Distractor *E*						
High span	0.009	0.025	0.018	0.031	0.024	0.053
Low span	0.026	0.046	0.021	0.039	0.033	0.050
Target absent						
Distractor *O*						
High span	0.017	0.031	0.005	0.016	0.006	0.018
Low span	0.016	0.028	0.011	0.024	0.009	0.025
Distractor *E*						
High span	0.023	0.037	0.017	0.031	0.013	0.028
Low span	0.030	0.048	0.014	0.035	0.011	0.029
Less organized arrays						
Target present						
Distractor *O*						
High span	0.022	0.042	0.009	0.030	0.019	0.029
Low span	0.015	0.040	0.016	0.039	0.025	0.057
Distractor *E*						
High span	0.016	0.032	0.023	0.042	0.030	0.048
Low span	0.021	0.048	0.027	0.042	0.058	0.081
Target absent						
Distractor *O*						
High span	0.010	0.026	0.007	0.016	0.003	0.014
Low span	0.014	0.033	0.011	0.024	0.007	0.023
Distractor *E*						
High span	0.016	0.032	0.015	0.029	0.008	0.028
Low span	0.028	0.048	0.019	0.035	0.016	0.052

Moreover, low spans missed more targets than did high spans, *F*(1, 115) = 3.97, η^2_P = .03, and a significant WM Span X Array Type X Array Size interaction, *F*(2, 230) = 3.71, η^2_P = .03, reflected that low spans had higher error rates than high spans in the largest, less organized arrays but also in the smallest, more organized arrays. To follow up on this interaction, we con-ducted a WM Span X Array Size ANOVA for more organized arrays, which yielded only a main effect of WM span, *F*(1, 115) = 4.43, η^2_P = .04, reflecting an approximate 1% span difference in accuracy. A corresponding ANOVA for less organized arrays yielded only a marginal WM Span X Array Size interaction, *F*(2, 230) = 2.55, *p* = .08, η^2_P = .02, reflecting less than a 1% span difference in accuracy for smaller arrays and a 1%–2% difference for larger arrays. Unlike the RT analyses, then, error rates

did yield evidence for span differences in search, but they were very small in magnitude. Remaining significant effects from the omnibus ANOVA were a main effect of array type, $F(1, 115) \sim 4.43$, ~p 2 ~ .04, and Array Type X Array Size, $F(2, 230) = 4.01$, $\eta^2_P = .03$, and Distractor Type X Array Size interactions, $F(2, 230)$ 6.56, $\eta^2_P = .05$.

Target-Absent Trials

ANOVA indicated that subjects committed more false alarms on distractor *E*s than on *O*s, $F(1, 115) = 19.10$, $\eta^2_P = .14$, and on larger than on smaller arrays, $F(2, 230) = 13.48$, $\eta^2_P = .10$. No effects involving WM span were significant (largest $F = 2.32$, $p > .13$, $\eta^2_P = .01$, for the WM Span X Array Type interaction), and there were no other significant effects. The evidence for WMC-related error differences in search was thus limited to misses, not false alarms.

Reliability

Null WM span effects in visual search RTs may indicate that there is no fundamental relation between WM and visual search or, instead, that either WM span or search performance was not measured reliably. The version of OSPAN we used is reliable (see Kane et al., 2004), but attention tasks used in experimental investigations often are not (e.g., Friedman & Miyake, 2004); this is particularly true when the effects of interest are difference scores, such as search slopes. To assess reliability of our visual search RTs, we computed Cronbach's alpha in two different ways. First, we calculated it across the 12 RTs for each subject in conditions involving distractor *E*s, or inefficient search (2 array types X 3 array sizes X 2 target-present/-absent trials), and found that a = .87. Second, we calculated it across the four RT search slopes involving distractor *E*s (2 array types X 2 target-present/-absent trials), with slopes computed simply as the difference score between Array Size 16 and Array Size 1, and found that a = .79. Both of these reliability estimates are satisfactory, and, coupled with our reasonably large sample size, they suggest that the null span effects on search were not due to measurement problems.

Discussion

Experiment 1 replicates our pilot results with a larger subject sample and two different versions of the search task. As measured by the OSPAN task, individual differences in WMC were rather spectacularly unrelated to the speed and efficiency of visual search. Whether the stimulus displays were highly organized or less organized, low spans identified the presence and absence of search targets as quickly as did high spans, even in very large arrays. The lack of a WMC effect cannot be attributed to insensitive or unreliable measures, because our tasks yielded large search slopes in both of our inefficient conditions (*F*s among *E*s in more organized and in less organized arrays), and these slopes were reliably measured, as estimated by internal consistency statistics. Moreover, our samples of high and low spans were reasonably large relative to others in this research domain (with 50–60 subjects each), especially with all the remaining variables manipulated within subjects. The only suggestion of a WMC-related search effect was in misses, with low spans generally missing 0.5%–2% more targets than did high spans across conditions. This is a very small absolute difference, and, given the high accuracy rates overall (most conditions **~~!!** 98%) and the general insensitivity of this span difference to array size, we see only very weak evidence that the executive processes involved in WMC are also engaged during visual search.

Of course, one must be concerned that we are arguing to accept the null hypothesis. We believe that we are justified in doing so, given the reliability of our measures, the internal replication of a null effect between more and less organized arrays, and the external replication of the null effect from our pilot data. Nonetheless, to make a more compelling claim that we put forth a "good effort" (Frick, 1995, p. 135) to find any effect that might actually be there, we seek to replicate these findings again in yet a larger sample and in a different set of search tasks.

EXPERIMENT 2

Experiment 2 tested nearly 300 subjects in two WMC tasks and two visual search tasks. We included a second WMC task to increase the validity of our classification of subjects, and we used two visual search tasks that

differed in the surface characteristics of their stimuli and also in their likely attention demands. More-over, we warped our stimulus display matrices to appear even more random than the less organized arrays from Experiment 1; the resulting arrays were much more similar to those regularly used in the visual search literature. Finally, to quantify the strength of any relation between WMC and search, we measured correlations between the full range of WMC scores and search-performance measures in the sample, in addition to conducting ANOVA-based comparisons of the extreme groups of high- and low-WMC subjects.

The search tasks we used here were a conjunction search task, which asked subjects to search for a red vertical bar amid red horizontal and green vertical bars, and a spatial configuration search, which asked subjects to search for an *F* among *E*s and rotated *T*s. We thought it wise to investigate conjunction search, if only because of its prominence in the visual search literature following Treisman and Gelade (1980; see also Corcoran & Jack-son, 1979; Treisman, 1977). Conjunction search is also of interest because, in some sense, it might be considered a more complex task than the feature-absence search we assessed in Experiment 1, with attention being required to bind color and orientation features together into objects rather than just identifying a missing part. At the same time, conjunction search often yields very shallow search slopes (for reviews, see Treisman & Sato, 1990; Wolfe, 1994, 1998a) and substantially shallower than those we found in Experiment 1. In either case, whether conjunction search proves more or less demanding than feature-absence search, Experiment 2 will help generalize our Experiment 1 findings while making closer contact to the larger visual-search literature.

We chose to investigate spatial configuration search as well because it afforded an opportunity to seek WMC-related differences in a search task that was likely to be especially difficult. Subjects searched for a target *F* among both *E*s and tilted *T*s (90° to the left). To identify an *F* among these distractors, one cannot simply search for either the absence or the presence of a single feature. An *F* lacks the bottom horizontal bar of an *E*, but so does a tilted *T*; the *F* has a top horizontal bar that a tilted *T* lacks, but so does an *E*. Thus, subjects are forced to spatially combine all individual features to identify the target, and the resulting search slopes are very steep (Wolfe, 1998b). Moreover, Huang and Pashler (2005) have demonstrated that, unlike even quite difficult feature or conjunction searches, slopes for spatial configuration searches cannot be attributable solely to statistical decision noise that accumulates over larger stimulus arrays (Lu & Dosher, 1998; Palmer, Ames, & Lindsey, 1993). In a preparation using brief masked displays and *d'* as the dependent measure, Huang and Pashler found that only spatial configuration searches benefited from presenting the two halves of each array in rapid succession rather than the entire array all at once. Attention thus appears to be a limiting factor to spatial configuration search performance, so it provides an important testing ground for our executive attention theory of WMC.

Method

WMC Screening

We individually screened 344 undergraduates (ages 18–35), from the same sources as Experiment 1, for WMC using two span tasks, OSPAN and Reading Span (RSPAN; Daneman & Carpenter, 1980). The OSPAN task was identical to that in Experiment 1. The RSPAN task presented series of two to five unrelated sentences to judge, interpolated with two to five isolated letters to memorize (for more details, see Kane et al., 2004). Subjects first read each sentence aloud (e.g., "Andy was stopped by the policeman because he crossed the yellow heaven"), judged whether the sentence made semantic sense (e.g., "no"), and then immediately read the letter aloud (e.g., "R"). As in OSPAN, the experimenter then advanced the screen to the next sentence–letter pair or recall cue. At the recall cue, subjects wrote the letters that had appeared in the trial in serial order. Also as in OSPAN, three trials were presented at each length (two to five) in the same pseudorandom order for all subjects.

In contrast to Experiment 1, we scored each span task by averaging the percentage of items recalled in correct serial position for each trial in the task (see Kane et al., 2004). Thus, scores were expressed as proportions, ranging from .00 to 1.00. We have found this scoring method to correlate very strongly with others (*r*s = .90 and higher), including that used in Experiment 1, but it yields more normal, less positively skewed distributions in both university and community populations (Conway et al., 2005; Kane et al., 2004).

Visual Search

Subjects

Two hundred ninety-seven subjects completed both the WM screening and the visual search sessions (and were at least 85% accurate in solving the OSPAN operations and judging the RSPAN sentences) within the same academic semester. In this experiment, top and bottom quartiles (high spans and low spans) were based on a *z* score composite of proportional OSPAN and RSPAN scores.

Design

The design was a 2 X 2 X 3 X 2 mixed-model factorial, with WM span (high, low) as a between-subjects variable, stimulus type (letters, bars) manipulated within subjects and between trial blocks, and array size (2–4, 8–10, 17–19) and trial type (target present, target absent) manipulated within subjects and within trial blocks. We randomly assigned subjects to one of two search-type order conditions: letter search first versus bar search first.

Apparatus and Materials

An E-Prime 1.0 program, run on Dell Optiplex GX110 computers, presented the stimuli for the search task on a 17-in. (43-cm) color monitor and collected response latency and accuracy data. As in Experiment 1, we created letter stimuli in Microsoft Paint, in this case using a black Zurich Ex Bt font (a sans serif font) letter *E*, with the center horizontal bar lengthened such that it extended the same distance as the top and bottom bars; all lines were drawn 1 mm thick. The *E* (a distractor stimulus) measured 7 X 5 mm. We created a horizontally tilted *T* (the other distractor type) by removing both the top and bottom horizontal bars of the *E* and an *F* (the target stimulus) by removing the bottom bar of the *E*. We created the bar stimuli in a similar manner, removing all except the vertical bar of the *E* (7 X 1 mm), changing the color to red or green (using standard colors in the Paint program: for red bars, hue = 0, saturation = 240, luminance = 120, red = 255, blue = 0, green = 0; for green bars, hue = 80, saturation = 240, luminance = 60, red = 0, blue = 0, green = 128), and centering the bar within the 15 X 11 pixel space that the letters had occupied. The vertical red bar served as the target stimulus; the vertical green bar and horizontal red bar served as distractor stimuli.

All stimuli appeared within an irregular 7 X 7 matrix, for a total of 49 possible locations. We made the grid of possible locations irregular (more irregular than those used in Experiment 1) in several ways. Rows 1, 4, and 7 were offset to the right by the equivalent of 2.5, 2.3, and 3.0 locations, respectively, and Rows 2, 5, and 6 were shifted to the left by 3.25, 2.5, and 1.25 locations, respectively. The result was that none of the possible locations lined up vertically. We then moved individual locations vertically (maximum = 11 mm) and horizontally (maximum = 8.5 mm), with the requirements that no two locations were less than 1.5 mm away from each other in any direction and that any 2 adjacent horizontal locations were no more than 7.5 mm apart. The largest horizontal and vertical extents of any trial display were 103 mm and 75 mm, respectively (see Figure 4 for examples). We assigned numbers to possible locations, in ascending order, from left to right, by their previous row location, and we used a computerized random number generator to select the locations for particular stimuli on each trial (all subjects saw the same stimulus arrays, but in a different random order).

Subjects saw 96 trials in the bar search block and 96 trials in the letter search block. In each block, 48 trials presented a target, and 48 did not. Within target-present and target-absent conditions, 16 trials presented a small number of distractors (half the trials with 2 distractors, and half with 3), 16 trials presented a medium number of distractors (half with 8, half with 9), and 16 trials presented a large number of distractors (half with 17, half with 18). Subjects also completed 24 practice trials preceding each block, which approximately balanced all the experimental conditions.

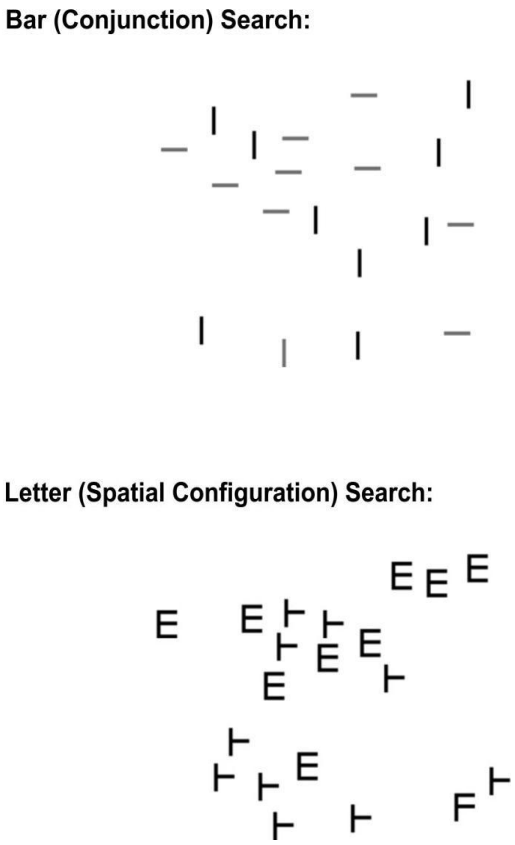


Figure 4. ***Examples of bar (conjunction) search and letter (spatial con-figuration) search stimulus arrays from Experiment 2. The gray bars in the figure appeared as red bars on screen, and the black bars in the figure appeared as green bars on screen. Subjects searched the bar arrays for a target vertical red bar and searched the letter arrays for a target F.***

Procedure

The only difference from Experiment 1 was that in the letter search task subjects reported whether an *F* appeared in each display by pressing either the *z* (*yes*) or the *m* (*no*) key on the keyboard and that in the bar-search task subjects similarly reported whether a vertical red bar appeared in the display.

Results

Subjects

Data from 15 subjects were dropped, 5 because of illness (excessive coughing or sneezing during the visual search session), 3 because of self-reported color blindness, and 7 because of computer error. This left a total of 282 subjects in the analyses, of which the top and bottom quartiles on the WM composite were classified as high and low spans, respectively. Because the bar and letter search tasks make such different attentional demands (Huang & Pashler, 2005), we analyzed their data separately.

To make contact with Experiment 1 as well as other recent investigations of WMC-related differences in attention-control tasks, we report ANOVAs to contrast performance of the extreme groups of high- and low-WM span subjects. In addition, we then report the correlation between WM span and search slope, using the full range of WM span scores from the sample of 282 subjects. Here, as in Experiment 1, we calculated slope as the difference score between the largest arrays (17–19 stimuli) and the smallest arrays (2–4 stimuli).

WMC Screening

The OSPAN and RSPAN scores were determined by the mean proportion of items recalled correctly across all sets for each span task. The mean performance on the OSPAN and RSPAN tasks, on the basis of the 282 participants' data, was 0.617 (*SD* = 0. 144) and 0.668 (*SD* = 0.139), respectively. The span measures correlated

at $r(282) = .609$. The proportion span scores were converted (separately for each measure) into *z* scores, and these were then aver-aged to create the composite WM score. For extreme-group span analyses, high- and low-WM span groups were determined by the upper and lower quartiles, respectively, of the composite WM scores. The cutoff for high spans' *z* scores was greater than 0.669, and the cutoff for low spans' *z* scores was lower than -0.608.

Bar Search (Conjunction Search) Task

RTs

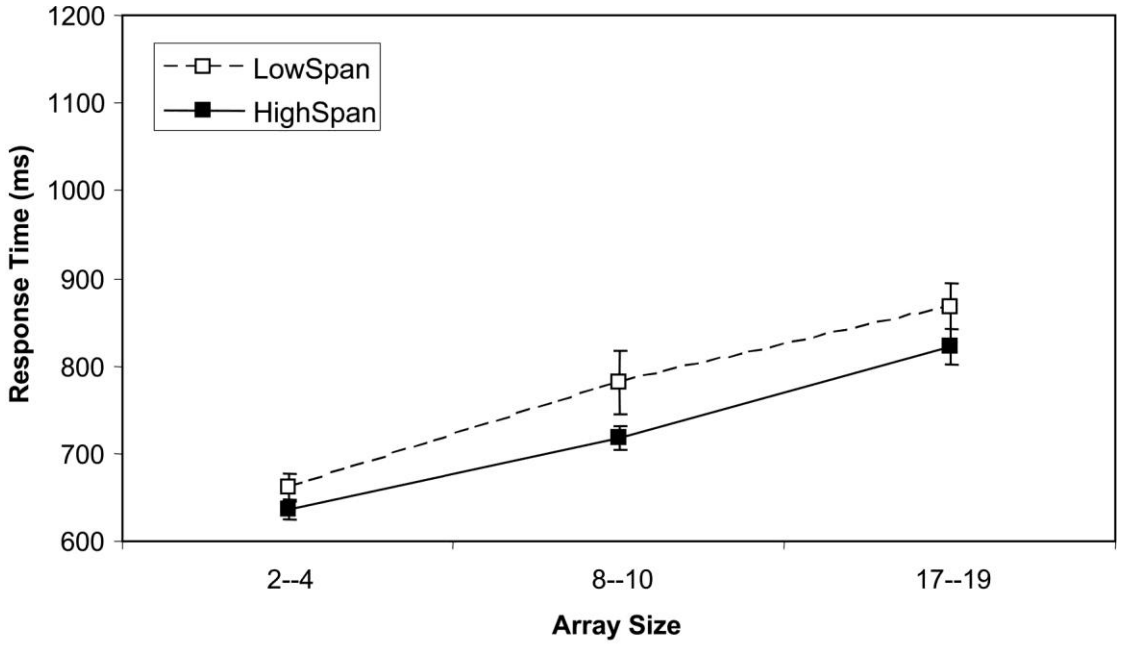
Figures 5A and 5B present the means of high- and low-span subjects' median RTs for the bar search task, by array size, for target-present and target-absent trials, respectively. A 2 (WM span group) X 3 (array size) ANOVA on target-present RTs indicated that search times increased with array size, $F(2, 276) = 155.68$, $\eta^2_P = .53$, but, despite this large effect, high- and low-WM-span groups did not differ in overall RTs, $F(1, 138) = 2.35$, $p = .13$, and WM span did not interact with array size, $F(2, 276) = 1.35$, $p = .26$. As expected, then, the correlation between WM span and target-present RT search slope in the full sample was nonsignificant and near zero, $r(282) = —.04$, $p > .50$. The ANOVA on target-absent trials also yielded a significant effect of array size, $F(2, 276) = 200.58$, $\eta^2_P = .59$, but yielded neither a main effect of WM span, $F(1, 138) = 1.60$, $p = .21$, nor a WM Span X Array Size interaction, $F(2, 276) < 1$. The WM Span X Target-Absent Slope correlation was, again, near zero, $r(282) = —.03$, $p > .50$. As in Experiment 1, we found no evidence for WMC-related differences in visual search.[3]

The lack of a significant relation between WM span and search RT is again meaningful because both were measured reliably. Recall that OSPAN and RSPAN scores correlated at .609, indicating good reliability (along with previously reported coefficient alphas for these tasks of about .80; Kane et al., 2004). Regarding visual search reliability, when internal consistency was calculated across all 12 RT conditions (2 search tasks X 3 array sizes X target present/absent), a = .853, and when it was calculated across 4 RT search slopes (2 search tasks X target present/absent), a = .585. Although the latter value is somewhat low, it is considerably higher than the near-zero correlations between WM span and search slopes, and it suggests at least a modest ability to detect any span-related associations.

Error Rates

Table 2 presents the mean error rates, for high- and low-WM-span subjects, by array sizes and target-present/-absent trials, for both the bar and the letter search tasks. A 2 (WM span group) X 3 (array size) ANOVA on target-present trials indicated that subjects missed more red vertical targets as array size increased, $F(2, 276) = 22.02$, $\eta^2_P = .14$, and that low spans missed more targets than did high spans, overall, $F(1, 138) = 4.20$, $\eta^2_P = .03$. Of most importance, however, the WM span difference did not increase across array sizes, $F(2, 276) = 1.45$, $p = .24$. The correlation between WM span and target-present search slope in the full sample was also near zero and nonsignificant, $r(282) = —.053$, $p = .38$. The parallel ANOVA on target-absent trials indicated that false alarms increased across array sizes, $F(2, 276) = 4.44$, $\eta^2_P = .03$, and that low spans committed more false alarms than did high spans, $F(1, 138) = 3.81$, $\eta^2_P = .03$, but, again, the WM span difference did not increase with array size, $F(2, 276) = 1. 11$, $p = .33$. Accordingly, the full-sample correlation between WM span and target-absent search slope was also nonsignificant and near zero, $r(282) = .012$, $p = .84$. Low spans were slightly more error prone than high spans overall. However, the search process itself, as reflected by interactions with array size, was unrelated to WMC.

Unlike the RT data, however, null correlations cannot be interpreted easily here because of poor reliability. Although Cronbach's alpha calculated for error rates over all 12 search conditions (2 search tasks X 3 array sizes X target present/absent) was .767, which is adequate, the value calculated across the four slopes (2 search tasks X target present/absent) was not, with a = .115. We therefore assessed the individual correlations between WM span



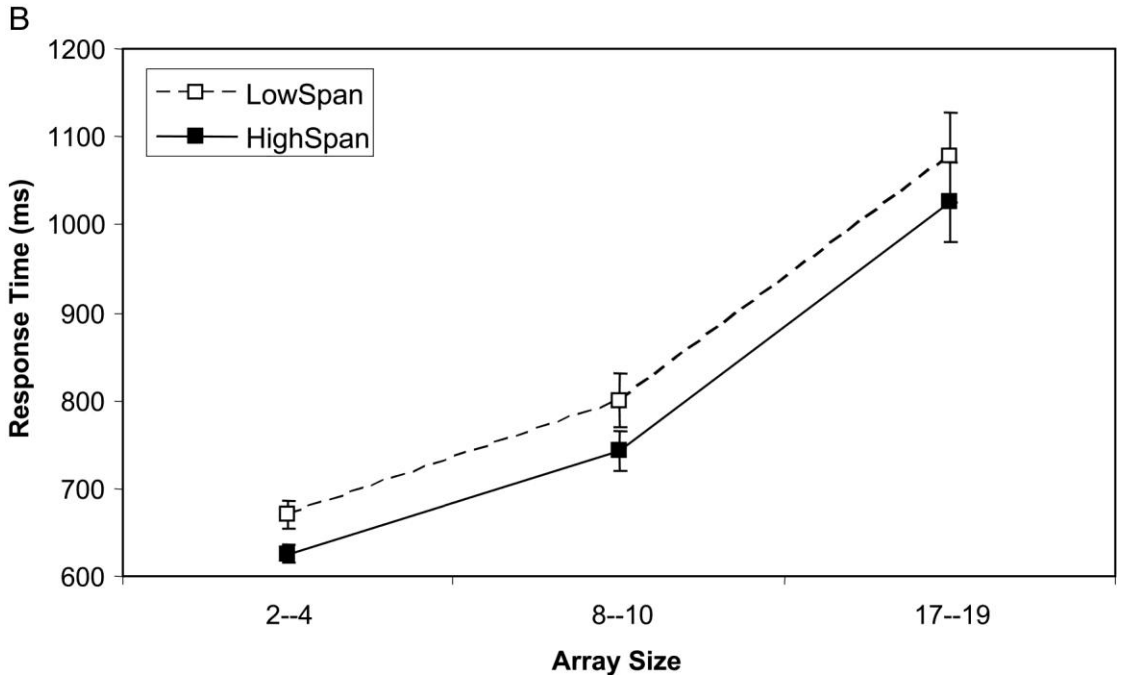


Figure 5. ***Mean response times (in milliseconds) for the bar (conjunction) search task from Experiment 2, by working memory span group (high span vs. low span) and array size. A: Data from target-present trials. B: Data from target-absent trials. Error bars represent standard errors of the means.***

and error rates on target-present and target-absent trials for small, medium, and large arrays separately rather than using the unreliable slope values. Nonetheless, even these correlations were generally near zero and nonsignificant. The only significant correlations were for target-present medium arrays, $r(282) = -.128$, and target-absent medium arrays, $r(282) = -.139$, although these isolated correlations were obviously rather weak and their 95% confidence intervals both included zero.

Letter Search (Spatial Configuration Search) Task

RTs

Figures 6A and 6B present the means of high- and low-span subjects' median RTs for the letter search task, by array size, for target-present and target-absent trials, respectively. A 2 (WM span group) X 3 (array size) ANOVA on target-present RTs indicated that subjects took more time to find targets as array size increased,

$F(2, 276) = 568.34$, $\eta^2_P = .80$. However, despite this large effect, high- and low-WM-span subjects did not differ in overall RTs, $F(1, 138) = 1.49$, $p = .23$, or in their RT increase over array sizes, $F(2, 276) = 1.63$, $p = .20$. Across the full range of WM span scores, the correlation between WM span and the letter search RT slope across array sizes was weak and only marginally significant, $r(282) = -.102$, $p = .09$. A parallel ANOVA on target-absent trials also indicated a very large effect of array size, $F(2, 276) = 469.02$, $\eta^2_P = .77$, but no significant main effect or interaction involving WM span (all Fs < 1). The correlation between WM span and RT search slope was near zero, $r(282) = -.018$,$p > .70$. Therefore, only target-present trials hinted at a weak relation between WMC and search, but even this correlation did not reach statistical significance with a sample of over 280 subjects, and the corresponding interaction was not nearly significant by the extreme-groups ANOVA.

Table 2
Mean Search Error Rates for Experiment 2

Target presence and working memory span	Array size: 2–4 *M*	2–4 *SD*	8–10 *M*	8–10 *SD*	17–19 *M*	17–19 *SD*
Bar-search (conjunction-search) task						
Target present						
High span	0.030	0.055	0.033	0.051	0.062	0.077
Low span	0.043	0.095	0.064	0.108	0.095	0.108
Target absent						
High span	0.016	0.033	0.004	0.015	0.009	0.029
Low span	0.030	0.070	0.023	0.075	0.017	0.050
Letter-search (spatial configuration search) task						
Target present						
High span	0.025	0.048	0.054	0.059	0.120	0.104
Low span	0.038	0.050	0.082	0.102	0.163	0.149
Target absent						
High span	0.009	0.024	0.004	0.016	0.014	0.030
Low span	0.019	0.037	0.015	0.038	0.024	0.043

Error Rates

A 2 (WM span group) X 3 (array size) ANOVA on target-present trials indicated that subjects missed more targets as array size increased, $F(2, 278) = 72.20$, $\eta^2_P = .34$, and that low spans missed more targets overall than did high spans, $F(1, 138) = 6.13$, $\eta^2_P = .04$. It is important to note that, as in the bar search task, the WM span difference in errors did not increase across array sizes, $F(2, 276) = 1.33$, $p = .27$. The correlation between WM span and target-present search slope in the full sample was only marginally significant, $r(282) = -.096$, $p = .11$. A parallel analysis on target-absent trials similarly showed that subjects committed more false alarms as array size increased, $F(2, 276) = 3.70$, $\eta^2_P = .03$, and that low spans committed more false alarms overall than did high spans, $F(1, 138) = 6.95$, $\eta^2_P = .05$. As with misses, the span difference in false alarms did not increase across array sizes, $F(2, 276) < 1$. The WM Span X Target-Absent Slope correlation was near zero, $r(282) = -.011$, $p = .84$. Because these error-rate search slopes were not reliable (see above), we also tested the correlation between WM span and each of the six bar search conditions individually (3 array sizes X target present/absent). The only significant correlations ($p < .05$) emerged from target-present trials with large arrays and target-absent trials with medium arrays, but these correlations were still relatively weak ($rs = -.119$ and $-.142$, respectively). (Of all the correlations we report for Experiment 2, this correlation of -.142 is the only one with a 95% confidence interval that did not include zero; even here, however, the upper limit of the confidence interval, -.026, was very near zero). Error rates thus provide little evidence for an association between WMC and visual search individual differences.

Discussion

The two visual search tasks we used in Experiment 2, conjunction search and spatial configuration search, suggest that WMC is unrelated to search efficiency. In conjunction search, for which the target was a vertical red bar amid red horizontal and green vertical bars, low spans identified the presence and absence of targets as rapidly as did high spans, and low and high spans showed equivalently inefficient searches across increasing set sizes. In the full sample of 282 subjects, WMC correlated nonsignificantly with target-present and target-absent RTs. Similarly, in spatial configuration search, for which the target was an *F* among *E*s and tilted *T*s and search slopes were dramatic, no differences emerged between high- and low-WMC subjects in either target-present or target-absent RT slopes. Although the correlation between WMC span and target-present search slopes was marginally significant in the full sample, with lower spans associated with larger slopes, the lack of conventional significance is telling given the very large sample size. Moreover, the two variables shared only 1% of their variance.

Error rates provided no more convincing evidence for a WMC– search link. As in Experiment 1, low spans generally committed more errors than did high spans, but the span groups did not differ in error-rate slopes across set sizes, indicating that the accuracy differences were not related to the search process itself. Perhaps because error rates were again generally low (especially for false alarms), the correlational data for error rate slopes were unreliable and thus not interpretable. However, WMC was also uncorrelated with error rates in nearly every experimental condition considered in isolation.

If the executive attention processes that are associated with WMC make any contribution to visual search, it must be a very minor one. In two feature-absence search tasks (Experiment 1), a conjunction search task (Experiment 2), and a spatial configuration search task (Experiment 2), the most compelling evidence for a relation between WMC and search performance is a single, marginally significant correlation reflecting only 1 % shared variance. It seems quite clear that in standard visual search tasks that yield substantial RT slopes but make no particular demands on the control of prepotencies, individual differences in WMC are largely irrelevant to performance. Prior work demonstrating WMC associations with Strooplike tasks (e.g., Kane et al., 2001; Kane & Engle, 2003; Long & Prat, 2002; Unsworth et al., 2004) suggests that if researchers introduced such prepotencies or habits into visual search—for example, by providing extensive practice with consistently mapped stimuli and then reversing target and distractor roles—then span-related differences should emerge. However, this kind of manipulation would be at odds with the point of these experiments, in that we are trying to determine where in the semantic space of attention tasks the relation between WMC and attention control breaks down.

Although some research suggests that spatial configuration search, at least, is sensitive to attentional limitations (Huang & Pashler, 2005), the top-down control implemented in models such as guided search (Wolfe, 1994) seems considerably different than the phenomenologically effortful or willful control processes thought to be applied in Strooplike tasks that require active goal maintenance and habit restraint (e.g., De Jong, Berendsen, & Cools, 1999; Kane & Engle, 2003; West & Alain, 2000; see also Monsell, 1996). In guided search, a subject's knowledge of the target features contributes activation to corresponding features in the master map, making it more likely that attention will be drawn (guided) to the target's location before other locations. Here, then, attention is thought to be exogenously pulled, more or less automatically, across descending peaks of activation in the master map. Endogenous, strategic control over a willful pushing of attention is presumed not to occur under most circumstances.

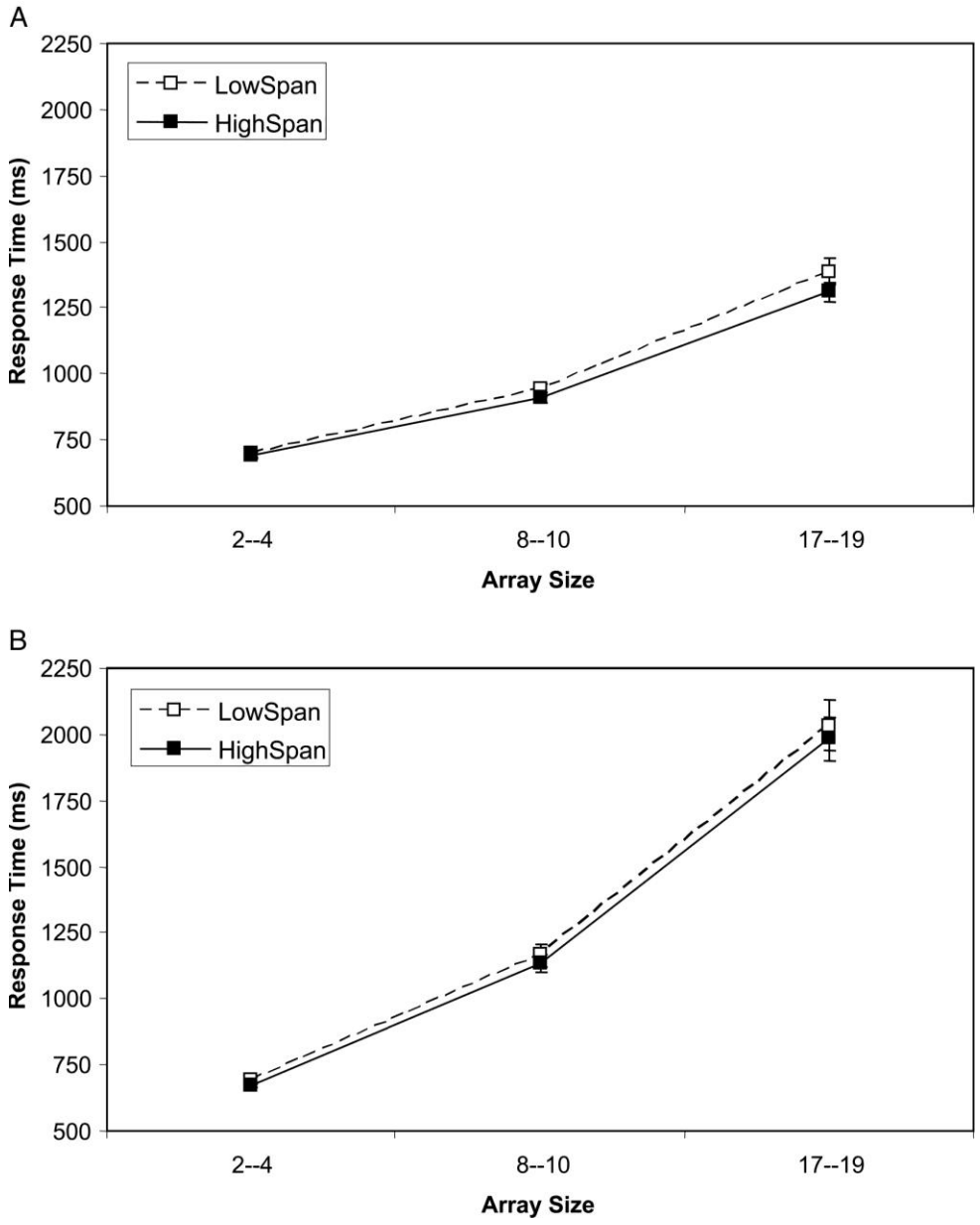


Figure 6. ***Mean response times (in milliseconds) for the letter (spatial configuration) search task from Experiment 2, by working memory span group (high span vs. low span) and array size. A: Data from target-present trials. B: Data from target-absent trials. Error bars represent standard errors of the means.***

Evidence for this claim was provided by Wolfe et al. (2000), who reported data from four types of tasks in which subjects were asked to move their attention in a specified path around a circular display of eight letters. In one version of the *command* task, subjects saw a sequence of eight masked frames on each trial, and the target letter (an *N* or a *Y*) could appear in only the clockwise position *N* in Frame N, with *N* = 1 corresponding to the 12:00 position. Thus, subjects had to move their attention in a clockwise direction across frames to identify the target. Across trials, the authors varied presentation rate for each subject to determine the minimum rate allowing 70% accuracy. Wolfe et al. found that presentation rates of some 200 ms per frame were necessary to maintain criterion accuracy (and other, similar methods required equivalent or larger rates). In contrast, in a control, *anarchic* condition in which the target appeared on every frame but in a different random location on each, subjects needed only about 100 ms per frame to reach criterion (other versions of the control task yielded even faster rates). Paradoxically, then, when subjects had no advance knowledge of where the target could be and so had no need to move attention strategically around the array, they identified targets much more quickly than when they had advance knowledge and a motivation to search strategically. These exceedingly slow

volitional search rates, of 100 –200 ms per item, suggest that search is not normally accomplished in this strategic way; slopes of 20 – 40 ms per item are considered normatively steep in typical tasks.

Wolfe et al. (2000) thus suggested that attention typically moves "anarchically" through search displays, guided by bottom-up and top-down sources of activation in a master map of spatial locations, not volitionally or strategically (for related findings, see Van Zoest, Donk, & Theeuwes, 2004). However, if endogenous pushing of attention can be elicited by some task contexts, as Wolfe et al. observed in their command task, would these control requirements be sufficient to yield WMC-related differences in performance, even in the absence of a requirement to restrain habit? A direct empirical comparison of exogenous versus endogenous control of attention during search motivates our Experiment 3.

EXPERIMENT 3

Subjects completed a version of the Wolfe et al. (2000) command search task and a version of their anarchic task, and we predicted WMC-related performance differences only in the command condition, which required endogenous control over spatial deployment of attention. The task presented static search displays of letters arranged in a circle, and subjects reported whether the first *F*-like stimulus they encountered as they moved clockwise through the display was an *F* or a backward *F*; the remaining distractors were *E*s, backward *E*s, *T*s tilted 90° to the right, and *T*s tilted 90° to the left. Because each display could present more than one *F* or backward *F*, subjects could only determine the target by searching in the clockwise direction, so volitional control over search was necessary. In this experiment, we also tested whether span differences in performance would be exacerbated by presenting additional noise, in the form of all-distractor rings of stimuli, at the same time as the search display. We hypothesized that the presence of distractor rings would increase the top-down demands to control competition from task-irrelevant stimuli (see Awh, Matsukura, & Serences, 2003). In particular, half the command trials were "clean," with no other stimuli displayed beyond the eight search stimuli, and half the trials were "noise" trials, presenting two distractor rings of eight stimuli (one ring internal to and one external to the target ring).

The anarchic condition simply presented only one *F* or back-ward *F* in each display (along with the forward and backward *E* and tilted-*T* distractors), so searching in a clockwise direction was not necessary to identify the target. All anarchic trials were clean trials. This condition served as yet another prototypical visual search task that, according to our findings so far, should not yield WMC-related differences.

Method

WMC Screening

We individually screened 120 undergraduates at the University of North Carolina at Greensboro (ages 18 –35) for WMC as in Experiment 2, but we attempted to further improve our measurements by adding a fourth trial at each set size two through five in the OSPAN and RSPAN tasks. Proportion scores, *z* score composites, and high- and low-span groups were calculated and defined as in Experiment 2.

Visual Search

Subjects

Of the 120 subjects screened, 118 completed the visual search session (and were at least 85% accurate in solving the OSPAN operations and judging the RSPAN sentences) within the same academic semester. Subjects were randomly assigned to one of two task orders, command task first or anarchic task first.

Command Search Task

Design. The design was a 2 X 2 X 2 X 8 mixed-model factorial, with span (high, low) as a between-subjects variable and display type (noise, clean), target type (*F*, backward *F*), and target location (1–8) manipulated within subjects and within blocks.

Apparatus and materials. Computer equipment and software were identical to those in Experiment 2, except that in the present experiment, subjects responded using the leftmost and rightmost buttons on a PST Serial

Response box (Model 2.0 Psychological Software Tools, Pittsburgh, PA). Letter stimuli were identical to those in Experiment 2, but we also created backward versions of the *E*, *F*, and tilted *T* by horizontally flipping each letter. The *F* and backward *F* served as targets for the search tasks (i.e., subjects reported which of the two targets was present on each trial), and the remaining letters served as distractors; subjects pressed the leftmost button for backward *F*s and the rightmost button for *F*s.

Stimuli appeared in three concentric rings with diameters measuring 10.2, 7.6, and 4.4 cm for the outer, middle, and inner rings, respectively. Eight stimulus locations were evenly spaced around each ring for a total of 24 locations per trial. The target always appeared in 1 of the 8 locations around the middle ring (see Figure 7), and subjects responded only to the first target (*F* or backward *F*) occurring clockwise from the 12:00 (top) position on the middle ring.

Subjects saw 112 clean trials and 112 noise trials within the same block of trials. Each clean trial presented one target and one false-target lure (i.e., an additional *F* or backward *F*) in two of the eight middle ring locations, plus six distractors chosen at random, with the constraint that no distractor appeared more than twice in the remaining locations. All other locations contained square dots (1 X 1 mm). Each target appeared in each of the eight locations seven times; across these seven trials, a false-target lure appeared one time in each of the seven remaining locations clockwise from the target. On trials in which fewer than seven locations were available because of the target position (e.g., when a target appeared at the 6:00, or fifth stimulus, location), each available location was populated with a lure equally often. When the target appeared in the eighth location, no lure was presented, and in its place another distractor was chosen such that no distractor appeared more than twice.

Noise trials were constructed in the same way, but they also presented stimuli in the inner and outer rings. Each trial presented one *F* and one backward *F* as false-target lures in both the inner and the outer rings. The locations of the lures were fixed, and one of each lure type was chosen at random separately for the inner and outer locations. For half the trials, lures appeared in Locations 2 and 6 on the outer ring and Locations 1 and 5 on the inner ring, and for the other half, the lures appeared in Locations 4 and 8 on the outer ring and Locations 3 and 7 on the inner ring. We counter-balanced lure locations so that they did not indicate the target's identity or location. The remaining locations not occupied by lures in the inner and outer rings were populated by distractors, chosen at random, with the constraint that no distractor appeared more than twice in each ring.

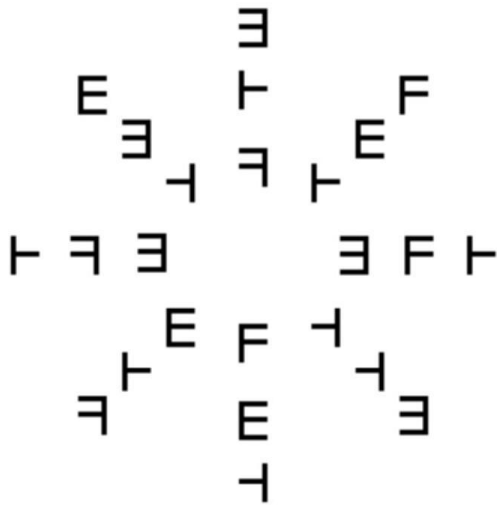


Figure 7. ***Example of a stimulus array from the command search task (noise trial). Subjects searched the middle ring, clockwise, for the first F-like stimulus and reported whether it was an F or a backward F. Stimuli on the outer and inner rings were distractors.***

Thirty-two trials served as command-search practice (2 target types X 8 target locations X 2 repetitions); half of these were clean trials, and half were noise trials. Subjects completed a single block of 448 experimental trials, with 224 trials (2 target types X 8 target locations X 7 lure locations X 2 false target types) each in the clean and noise conditions.

Procedure. Subjects first completed a block of 48 response-mapping practice trials, then the blocks of search-practice and experimental search trials. Each response-mapping trial presented one target in one of the eight middle ring locations, and these repeated three times each (2 target types X 8 locations X 3 repetitions). All other locations in the display contained 1 X 1 mm square dots.

Each search-practice and experimental trial first presented a blank screen for 500 ms and then a fixation display for 1,500 ms, which populated all 24 possible locations with 1 X 1 mm square dots. The search display then appeared and remained onscreen until response. We instructed subjects to use the dots in the fixation display as placeholders for the upcoming search items and to sustain focused attention on only the dots representing the middle ring (potential target) locations.

Anarchic Search Task

Design. The design was a 2 X 2 X 8 mixed-model factorial, with span (high, low) as a between-subjects variable and target type (*F*, backward *F*) and target location (1–8) manipulated within subjects and within block.

Apparatus and materials. The anarchic task used the same stimuli as in the command task. All trials presented eight stimuli in a ring shape (the same as the middle ring in the command task). Only one *F* or backward *F* was presented on each trial. Seven distractors (forward and backward *E*s and tilted *T*s) appeared in the remaining locations, chosen at random with the constraint that no distractor appeared more than twice on any given trial. Each of the two target types appeared in each of the eight locations around the ring four times for a total of 64 trials (2 target types X 8 locations X 4 repetitions). No dots were presented as placeholders either before or during any anarchic experimental trials.

Procedure. Subjects first completed a block of 32 response-mapping practice trials and then the 64 experimental search trials. The response-mapping trials presented one target alone in one of the eight locations around the ring, two times each for a total of 32 trials (2 target types X 8 locations X 2 repetitions). The seven remaining locations contained 1 X 1 mm square dots. Experimental trials first presented a blank screen for 500 ms, then a centered warning dot (1 X 1 mm) for 740 ms, and then the search display until response.

Results

Subjects

Of the 118 subjects tested, data from 18 were dropped from all visual search analyses: 2 nonnative English speakers, 1 with missing span data, 8 with computer errors during the search task, and 7 because of illness (excessive coughing or sneezing). This left 100 subjects with both WM span and visual search data in the analyses. Of these subjects, 16 (4 high spans, 7 middle spans, and 5 low spans) had error rates greater than 30% on command-task noise search trials, so their command-task data were excluded from analyses.

WMC Screening

The OSPAN and RSPAN scores were determined by the mean proportion of items recalled correctly across all sets for each span task. The mean performance on OSPAN and RSPAN, on the basis of the 100 participants' data, was 0.605 (*SD* = 0.131) and 0.636 (*SD* = 0. 128), respectively. The span measures correlated at .686. As in Experiment 2, composite WM scores were derived from *z* score composites of OSPAN and RSPAN. For extreme-group span analyses, high- and low-WM-span groups were determined by the upper and lower quartiles, respectively, of the WM composite scores, with the cutoff for high spans' *z* scores greater than 0.537 and the cutoff for the low spans' *z* scores lower than -0.565.

Visual Search

As in Experiment 2, we first present RT analyses and then error analyses, by extreme WM span groups (top vs. bottom quartiles), for each visual search task. We then follow those ANOVAs with correlations between search slopes and the full range of WM span scores in the sample. Following those analyses, we compare command search (clean trials) with anarchic search to test whether the command task appeared to make greater control demands than did the anarchic task, as predicted.

Command Visual Search Task

RTs. Means of median RTs in the command visual search task are presented in Table 3. A 2 (WM span group) X 2 (display type: clean vs. noise) X 8 (target location: 1– 8) X 2 (target type: backward *F* vs. *F*) mixed-model ANOVA, with WM span group as a between-subjects factor, indicated that clean trials were per-formed faster than noise trials, $F(1, 39) = 282.73$, $\eta^2_P = .88$, and RTs increased across clockwise target locations, $F(7, 273) = 772.30$, $\eta^2_P = .95$. As well, RTs to target *F*s were shorter than RTs to backward *F*s, $F(1, 39) = 91.06$, $\eta^2_P = .70$.

Despite these very large experimental effects, the only significant main effect or interaction involving WM span was a three-way interaction, depicted in Figure 8, of WM span with target location and target type, $F(7, 273) = 2.02$, $p = .05$, $\eta^2_P = .05$ (for all other span effects, *F*s < 1.7 1, *p*s > .198). Although the effects of WM span did not appear to be systematic, we conducted separate 2 (WM span) X 8 (target locations) mixed-model ANOVAs for the targets backward *F* and *F*. For the backward *F* trials, RTs increased across clockwise locations, $F(7, 273) = 665.60$, $\eta^2_P = .94$, but neither the main effect of WM span, $F(1, 39) = 1.14$, $p = .29$, nor the interaction of WM span with target location, $F(7, 273) < 1.0$, was significant. For *F* target trials, RTs also increased across locations, $F(7, 273) = 690.38$, $\eta^2_P = .95$, and, again, neither the main effect of WM span, $F(1, 39) = 1.29$, $p = .26$, nor the interaction of WM span with target location, $F(7, 273) < 1$, was significant. Thus, the small three-way interaction of WM span with target location and target type is not readily interpretable.

Table 3
Mean Search Reaction Times for the Experiment 3 Command Task

Working memory span and target type	Target location 1	2	3	4	5	6	7	8
Clean trials								
High span								
F target								
M	663	671	842	1,023	1,178	1,390	1,628	1,706
SD	111	97	107	110	138	196	213	239
Backward *F* target								
M	775	759	949	1,090	1,277	1,461	1,624	1,665
SD	120	103	144	142	136	203	211	217
Low span								
F target								
M	722	697	901	1,068	1,242	1,440	1,644	1,747
SD	209	150	139	195	256	258	236	304
Backward *F* target								
M	830	805	929	1,123	1,354	1,488	1,704	1,724
SD	278	167	152	188	258	269	271	329
Noise trials								
High span								
F target								
M	726	783	996	1,282	1,496	1,731	1,932	2,144
SD	156	168	132	191	209	256	262	267
Backward *F* target								
M	786	836	1,076	1,349	1,584	1,789	2,010	2,200
SD	158	147	169	149	191	251	190	300
Low span								
F target								
M	823	820	1,086	1,325	1,516	1,837	2,069	2,327
SD	341	188	213	269	284	331	369	502
Backward *F* target								
M	891	896	1,099	1,431	1,646	1,865	2,162	2,325
SD	285	217	177	278	287	339	354	475

Note. Reaction times are in milliseconds.

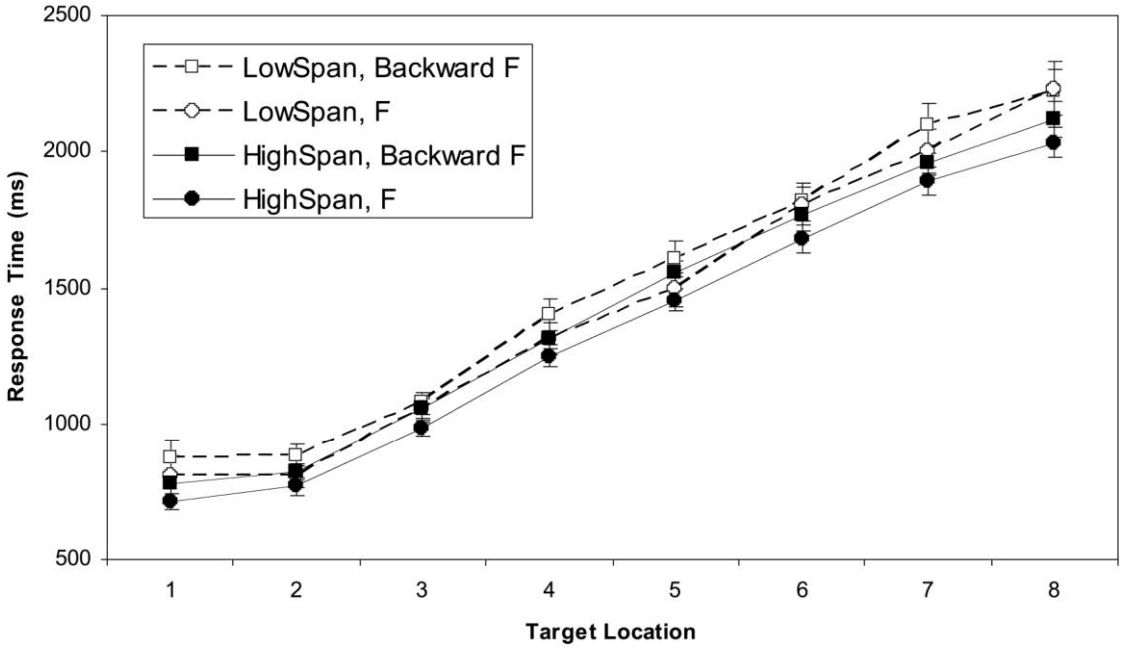


Figure 8. ***Mean response times (in milliseconds) for the command search task from Experiment 3, by working memory span group (high span vs. low span), target type (F vs. backward F), and clockwise target location. Error bars represent standard errors of the means.***

Remaining significant interactions from the omnibus ANOVA were as follows: (a) between display type and target location, $F(7, 273) = 77.78$, $\eta^2_P = .67$, with a greater increase in RT across clockwise target locations for noise than for clean trials; (b)between target location and target type, $F(7, 273) = 6.37$, p = .14, with *F*s eliciting a greater slowing across clockwise locations than did backward *F*s; (c) among display type, target location, and target type, $F(7, 273) = 2.14$, $\eta^2_P = .05$, with the slowing difference across locations between *F*s and backward *F*s being greater in noise than in clean trials.

Within the full sample, WM span correlated nonsignificantly and near zero with RT search slopes (i.e., the difference scores between Locations 8 and 1). This was true for clean *F* trials, $r(84) = .012$, $p = .92$; for clean backward *F* trials, $r(84) = .012$, $p = .91$; for noise *F* trials, $r(84) = -.069$, $p = .54$; and for noise backward *F* trials, $r(84) = -.035$, $p = .75$. As with the RTs from Experiment 2, these null correlations (all with 95% confidence intervals that included zero) are meaningful because we measured WM span and search slopes reliably. For search slopes, we calculated Cronbach's alpha by taking the slope difference score between Locations 1 and 7 and also between Locations 2 and 8. We did this separately for clean and noise trials and for *F* and back-ward *F* trials, which yielded eight slope values for each subject (a = .889). With coefficient alpha computed separately for clean and noise trials (four values each), as = .752 and .853, respectively. Clearly, these slope values were reliable enough to detect any possible correlations with WM span.

Error rates. Mean error rates on the command task are presented in Table 4 and were analyzed with a 2 (WM span) X 2 (display type) X 8 (target location) X 2 (target type) mixed-model ANOVA, with WM-span group as a between-subjects factor. Subjects made more errors on noise trials than on clean trials, $F(1, 39) = 4.70$, $\eta^2_P = .11$, and there was a significant effect of target location, $F(7, 273) = 7.22$, $\eta^2_P = .16$, with the greatest proportion of errors occurring at the first clockwise target location. Neither the main effect nor any interaction with WM span was significant (all *F*s < 2.67, *p*s > . 110). Remaining significant interactions from the omnibus ANOVA were between display type and target location, $F(7, 273) = 2.65$, $\eta^2_P = .06$, apparently driven by a large error rate on clean trials at target Location 7, and between target type and target location, $F(7, 273) = 4.24$, $\eta^2_P = .10$, apparently driven by stable error rates across locations for *F*s but declining rates for backward *F*s.

Within the full sample, WM span correlated nonsignificantly and near zero with error rate search slopes: For clean *F* trials, $r(84) = -.048$, $p = .66$; for clean backward *F* trials, $r(84) = -.006$, $p = .95$; for noise *F* trials, $r(84) = .079$, $p = .47$; and for noise backward *F* trials, $r(84) = .090$, $p = .42$. As in Experiment 2, however, our error slope data had questionable reliability (calculated in this experiment as we did for RTs). Cronbach's alpha across the eight slope values for clean and noise *F* and backward *F* trials was .677, which is arguably just adequate, but when alphas were calculated separately for clean and noise trials, they were only .322 and .566, respectively. These values, particularly the former, are low enough to cloud interpretation of the null correlation with WM span. Nonetheless, when we calculated a mean slope for each subject over the eight slope values that together yielded a reasonable reliability estimate, the slope correlation with WM span was still zero, $r(84) = .025$, $p = .823$ (again, 95% confidence intervals for all the WMC-error correlations included zero).

Table 4
Mean Search Error Rates for the Experiment 3 Command Task

Working memory span and target type	Target location 1	2	3	4	5	6	7	8
Clean trial								
High span								
F target								
M	0.048	0.007	0.007	0.010	0.031	0.020	0.065	0.037
SD	0.069	0.021	0.021	0.026	0.053	0.040	0.087	0.058
Backward *F* target								
M	0.068	0.037	0.034	0.041	0.034	0.034	0.061	0.014
SD	0.070	0.054	0.077	0.070	0.049	0.058	0.072	0.029
Low span								
F target								
M	0.046	0.011	0.004	0.011	0.021	0.029	0.054	0.054
SD	0.117	0.026	0.016	0.026	0.041	0.049	0.095	0.069
Backward *F* target								
M	0.068	0.018	0.011	0.011	0.036	0.025	0.036	0.014
SD	0.102	0.039	0.026	0.026	0.049	0.035	0.059	0.037
Noise trial								
High span								
F target								
M	0.051	0.054	0.028	0.034	0.051	0.048	0.035	0.061
SD	0.061	0.071	0.052	0.081	0.080	0.065	0.067	0.076
Backward *F* target								
M	0.088	0.031	0.046	0.030	0.033	0.057	0.047	0.037
SD	0.084	0.048	0.090	0.041	0.057	0.081	0.059	0.066
Low span								
F target								
M	0.058	0.025	0.015	0.013	0.025	0.016	0.018	0.054
SD	0.093	0.035	0.028	0.031	0.041	0.028	0.038	0.061
Backward *F* target								
M	0.086	0.029	0.019	0.016	0.042	0.016	0.033	0.011
SD	0.126	0.063	0.034	0.034	0.064	0.034	0.053	0.026

Anarchic Visual Search Task

RTs. Means of median RTs from the anarchic task are presented in Figure 9. We analyzed these data with a 2 (WM span) X 8 (target location) X 2 (target type) mixed-model ANOVA, with WM-span group as a between-subjects factor. An unexpected finding was that earlier clockwise locations elicited faster responses than did later ones, $F(7, 336) = 12.04$, $\eta^2_P = .20$ (but see also our subsequent analysis of command vs. anarchic task order, which seemed to moderate this location effect). RTs to *F*s were faster than RTs to backward *F*s, $F(1, 48) = 14.29$, $\eta^2_P = .23$, and target type interacted with target location $F(7, 336) \sim 3.47$, ~p 2 ~ .07, apparently because backward *F*s had longer RTs at later clockwise locations than did *F*s. WM span was associated with no significant effects; the only one that approached significance was an interaction with target type, $F(1, 48) = 2.66$, $p = .12$, $\eta^2_P = .05$. We investigated this interaction with separate ANOVAs for target *F*s and backward *F*s, but no main effect or interactions involving WM span were significant (largest $F = 1.48$, lowest $p = .17$). Within the full sample ($n = 100$), WM span showed no significant correlations with RTs at any of the target locations—with only one $r > .09$, for Location 8, $r(100) = .136$, $p = .18$; the correlation between WM span and mean search RT over all locations was $r(100) = —.02$, $p = .84$. These null correlations (all with 95%

confidence intervals that included zero) are meaningful be-cause anarchic search RTs were reliable: calculated across 16 variables (8 locations X 2 target types) and across 8 variables (locations only, collapsed over target type; Cronbach's as = .87 and .82, respectively).

Error rates. Mean error rates on the anarchic search trials are presented in Table 5 and were analyzed with a 2 (WM span) X 8 (target location) X 2 (target type) mixed-model ANOVA, with WM-span group as a between-subjects factor. Error rates did not differ across target locations, $F(7, 336) < 1$, but subjects made more errors on backward *F* targets than on *F* targets, $F(1, 48) = 3.84$, $p = .06$, $\eta^2_P = .08$, and target location interacted with target type, $F(7, 336) = 3.47$, $\eta^2_P = .07$, apparently reflecting higher error rates for backward *F*s than for *F*s at the first target location and lower error rates for backward *F*s than for *F*s at the latest clock-wise locations. WM span yielded no significant main effect or interactions, with all *F*s < 1. In addition, in the full sample, WM span showed no significant correlations to error rates at any of the target locations—only one *r*>.09: for Location 7,*r*(100) = —.148,*p* = .14; the correlation between WM span and mean error rate over all locations was *r*(100) = —.07, *p* = .49. Here, for the first time in our studies, these null correlations in error rates (all with 95% confidence intervals that included zero) are clearly meaningful because they were reliable: calculated across 16 variables (8 locations X 2 target types) or across 8 variables (locations only, collapsed over target type; Cronbach's a = .78).

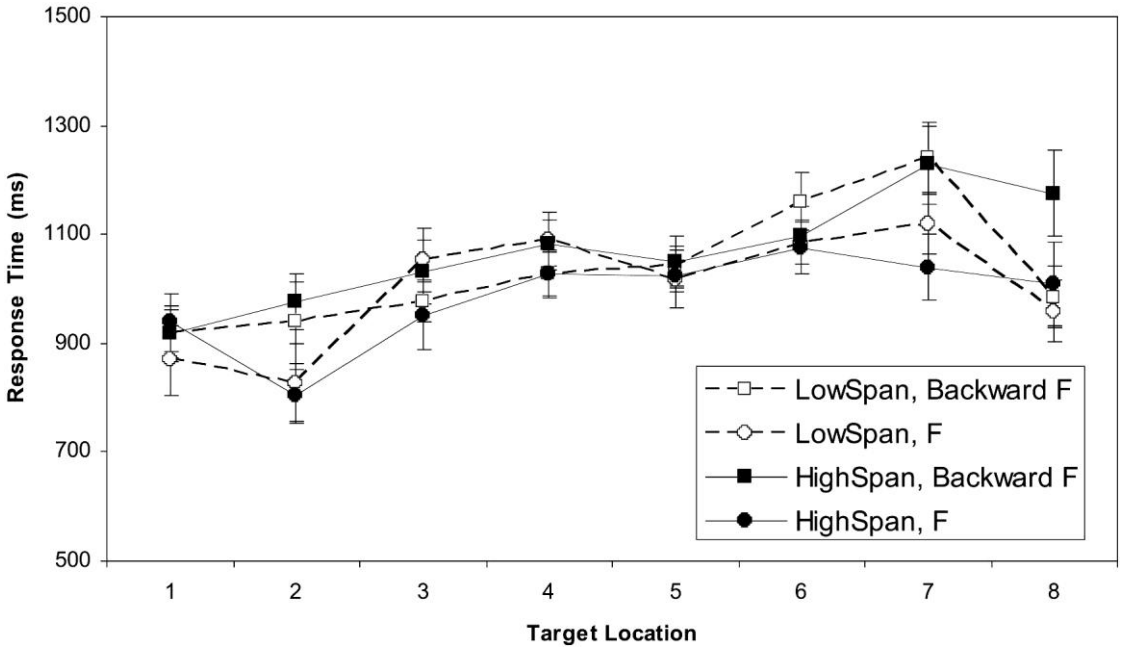


Figure 9. ***Mean response times (in milliseconds) for the anarchic search task from Experiment 3, by working memory span group (high span vs. low span), target type (F vs. backward F), and clockwise target location. Error bars represent standard errors of the means.***

Order Effects: Anarchic Versus Command Search

Two questions motivated the following RT analysis of order effects on performance of the anarchic and command (clean display) search tasks. First, is the command search task substantially different than the anarchic task? Only the former has been pro-posed to require the volitional movement of attention (Wolfe et al., 2000). Second, were task order effects responsible for the significant but unexpected target location effects found in the anarchic data (i.e., longer RTs across clockwise target locations)?

Mean RTs on the anarchic search and command search (clean) trials, by WM span, task order (anarchic first or anarchic second), and target location (1–8) are depicted graphically in Figures 10 (anarchic) and 11 (command clean). Because of random assignment to search task order, the WM-span group samples are unequal across

orders: Our analyses are based on data from 41 participants (anarchic first: 10 high spans, 9 low spans; anarchic second: 11 high spans, 11 low spans).

Table 5
Mean Search Error Rates for the Experiment 3 Anarchic Task

Working memory span and target type	Target location 1	2	3	4	5	6	7	8
High span								
F target								
M	0.000	0.020	0.020	0.020	0.010	0.060	0.030	0.040
SD	0.000	0.069	0.069	0.069	0.050	0.109	0.083	0.118
Backward *F* target								
M	0.050	0.030	0.040	0.050	0.020	0.020	0.060	0.020
SD	0.102	0.083	0.094	0.102	0.069	0.069	0.149	0.100
Low span								
F target								
M	0.010	0.020	0.020	0.000	0.000	0.060	0.020	0.020
SD	0.050	0.069	0.069	0.000	0.000	0.131	0.069	0.069
Backward *F* target								
M	0.060	0.020	0.020	0.070	0.030	0.010	0.000	0.000
SD	0.109	0.069	0.069	0.198	0.110	0.050	0.000	0.000

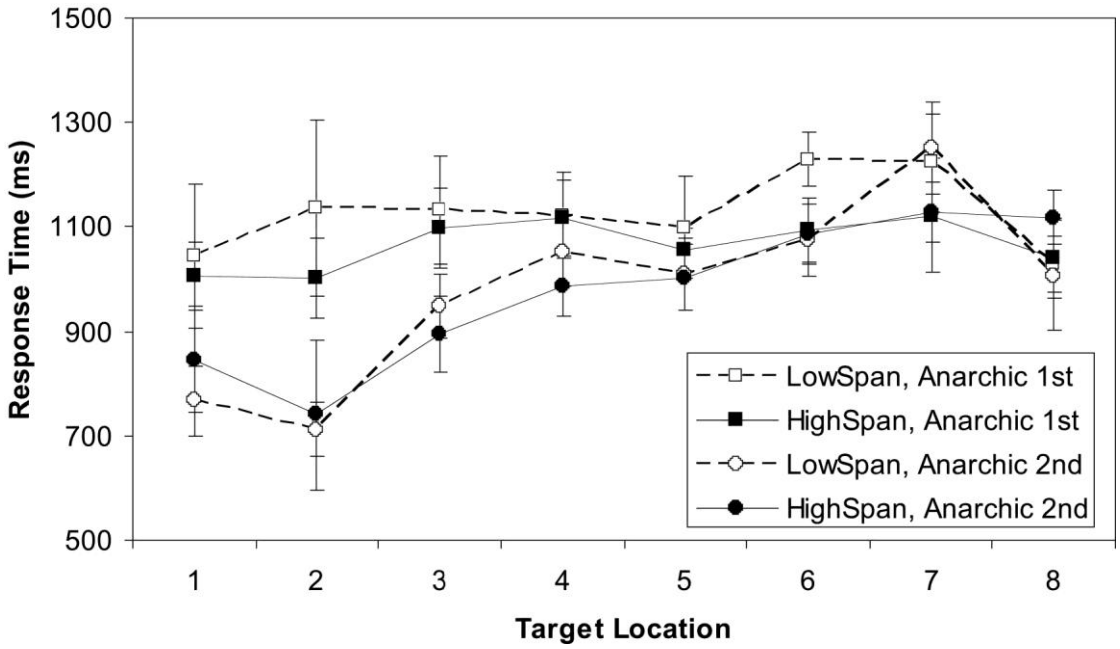


Figure10. ***Mean response times (in milliseconds) for the anarchic search task from Experiment 3, by task order (anarchic first vs. second), working memory span group (high span vs. low span), and clockwise target location. Error bars represent standard errors of the means.***

We conducted a 2 (WM span) X 2 (search task) X 2 (task order) X 8 (target location) mixed-model ANOVA, with WM-span group and task order as between-subjects factors (we report only the effects relevant to our questions). Command clean searches took significantly longer than anarchic searches, $F(1, 37) = 58.76$, $\eta^2_P = .61$, supporting the claim that performance of the command task is more controlled than performance of the anarchic task. Later clockwise target locations had longer RTs than earlier locations, $F(7, 259) = 192.24$, $\eta^2_P = .84$, but this effect was qualified by a Target Location X Task Order interaction, $F(7, 259) = 3.61$, $\eta^2_P = .09$,

with increasing RTs across clockwise locations when the anarchic task was performed after the command task but relatively constant RTs across locations when it was performed first. Search task interacted with target location, $F(7, 259) = 95.69$, $\eta^2_P = .72$, but this interaction was qualified by a three-way Task Order X Search Task X Target Location interaction, $F(7, 259) = 3.93$, $\eta^2_P = .10$. This seems to indicate that the target location effect was present in the command task, regardless of task order, but anarchic RTs increased across target locations only when they were per-formed after the command task. There were no significant main effects of WM span or interactions between WM span and the other variables (all *F*s < 2.02, *p*s > .163).

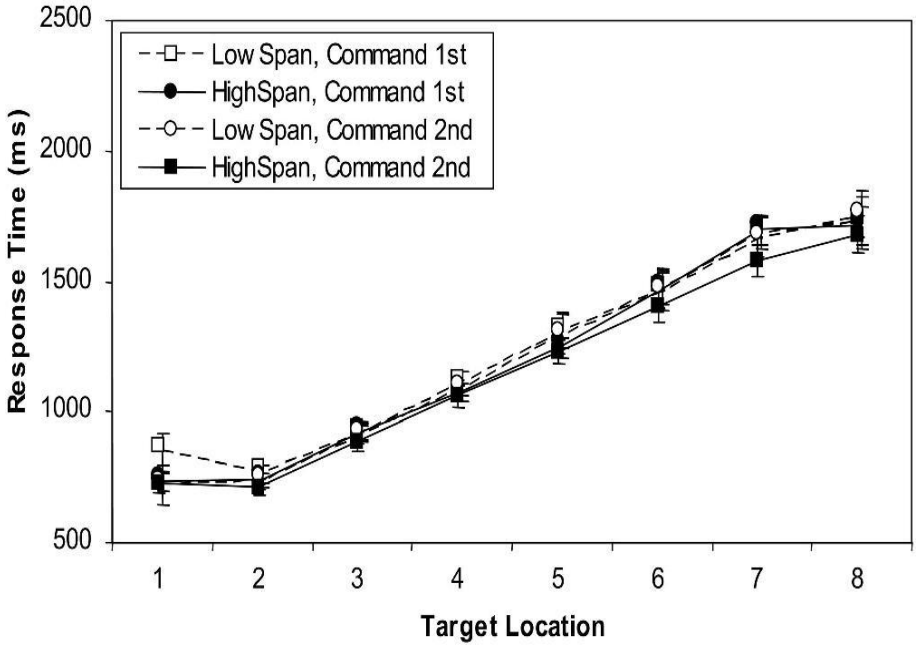


Figure 11. ***Mean response times (in milliseconds) for the command search task from Experiment 3, by task order (Command first vs. second), working memory span group (high span vs. low span), and clockwise target location. Error bars represent standard errors of the means.***

To further investigate the three-way interaction of task order, search task, and target location, we conducted separate Location X Order ANOVAs for the command and anarchic tasks. In the command task, early clockwise target locations had shorter RTs than did later ones, $F(7, 273) = 637.23$, $\eta^2_P = .94$, but neither the main effect of task order nor the interaction between target location and task order was significant (both *F*s < 1). Thus, previous anarchic task performance had no influence on the command task (see Figure 11). In the anarchic task, the main effect of target location was significant, $F(7, 273) = 10.93$, $\eta^2_P = .22$, as was the main effect of task order, $F(1, 39) = 4.63$, $\eta^2_P = .11$, but they were qualified by a significant Target Location X Task Order interaction, $F(7, 273) = 4.71$, $\eta^2_P = .11$ (see Figure 10). When the anarchic task was performed first, RTs were relatively stable over target locations, but when the command task was performed before the anarchic task, participants responded faster at early clockwise target locations. This suggests that some aspect of the command task response set (e.g., "Start searching at the 12:00 position") perseverated into the subsequent anarchic task.

Discussion

Experiment 3 was designed to increase the executive control demands of the visual search tasks without creating a habit-restraint demand similar to that imposed by Strooplike tasks. We did so in the command task by requiring subjects to search potential target locations in a clockwise order on every trial (Wolfe et al., 2000). On half the trials we also presented distractor noise stimuli in locations that were always irrelevant and never contained the target (Awh et al., 2003). However, neither variable elicited WMC-related individual differences in search. Although search slopes across successive clockwise locations were very large, low spans searched through these locations at the same rate as high spans. In conjunction with the data from the anarchic task we included, then, Experiment 3 has produced two more examples of null WMC effects on visual search.

Despite its lack of span effects, the command task did appear to elicit endogenous control over attention. Search latencies increased dramatically across clockwise locations (approximately 900– 1,000 ms over eight locations on clean trials, suggesting search slopes of more than 100 ms/item), regardless of whether subjects completed the task before or after the anarchic task. Subjects thus seemed to follow the task instructions to guide their search strategically. In contrast, the anarchic search task, which represented a more prototypical experimental paradigm with no volitional instructions, yielded relatively flat slopes across locations when it was performed as subjects' first task. Our basic findings from these tasks therefore replicate those of Wolfe et al. (2000).

When the anarchic task was performed second, however, it yielded large slopes across locations, much like the command task. Across Locations 2–7 (the locations that approximated a linear RT increase), search times increased approximately 400–500 ms, yielding slope values of approximately 80–100 ms per item. This finding suggests that the task set from the previous command task persisted into the anarchic task despite our instructions to subjects. Given this indication of set perseveration, it is surprising that high-and low-WMC subjects' performance did not differ in the anarchic task when they performed it after the command task. Prior work with antisaccade and Stroop tasks has found that low spans have more difficulty than high spans in abandoning a controlled task set that has built up over a long block of trials (Kane et al., 2001; Kane & Engle, 2003). For example, in the antisaccade paradigm, low spans persist more than high spans do in looking away from the flashing cue when the task demands shift from antisaccade (look away from the cue) to prosaccade (look toward the cue). Perhaps it is only in contexts in which the task set requires a strong prepotency to be restrained that low spans have particular difficulty abandoning such controlled sets. Future work should address this question more rigorously.

A second surprise, given previous findings, is the lack of WMC sensitivity to the noise manipulation in the command task. The presence of two distractor rings in the noise displays increased subjects' search times by several hundred milliseconds compared with clean displays, but low spans were no more slowed by these distractors than were high spans. In contrast, prior research with the Eriksen flanker task (Heitz & Engle, 2006; Reddick & Engle, in press) and with Egly and Homa's (1984) object-based orienting task (Bleckley et al., 2003; Bleckley & Engle, 2006) suggests that low spans less effectively constrain visual attention than do high spans. We suggest that the requirement to move attentional focus, versus fixating focus, may be the critical variable here. That is, previous evidence for span differences in visual interference susceptibility has emerged from tasks in which subjects must restrict their focus to a single static location or object amid distractors in other static locations. Our command task, in contrast, asked subjects to restrict their focus to a ring of stimuli while they were also required to move their attention along that ring. It seems as though high spans' advantage in restricting focus could not be maintained as that focus was pushed from item to item. We therefore speculate that the requirement to move spatial attention across particular locations in this experiment prevented all subjects from adopting a very restricted spatial focus, which had the side effect of making them more vulnerable to distractors than they would otherwise have been.[4] Just as high spans' normal superiority in dealing with memory interference is thwarted by the addition of a secondary task to perform, making high spans and low spans similarly vulnerable to interference (Kane & Engle, 2000; Rosen & Engle, 1997), perhaps the requirement to move attention prevented high spans from doing what they normally do to effectively restrict attentional focus.

However, this idea—and our data—seems to conflict with frequently reported findings that irrelevant distractors have very little influence on performance during inefficient visual search tasks (e.g., Gibson & Peterson, 2001; Lamy & Tsal, 1999; Theeuwes, 1991, 1992). These prior findings of null distractor effects seem to suggest that distractors receive little processing under high perceptual load (Lavie & Cox, 1997; Theeuwes, 2004). Our data, in contrast, show that two rings of irrelevant distractors had a substantial disruptive effect on search efficiency. We are unsure about how to resolve this apparent conflict. However, it may be important that our noise trials presented many distractors, as opposed to prior work that only presented one singleton distractor. In addition or instead, our command task differed from others in that it required strategic, sequential search across locations rather than anarchic search. Future work is required to resolve these secondary issues.

Of most importance, however, the key finding from Experiment 3 is that, even in contexts in which subjects had to endogenously control visual attention by moving it strategically through search arrays, high- and low-WMC subjects performed equivalently. Even top-down, controlled visual search did not elicit WMC-related individual differences.

GENERAL DISCUSSION

In three experiments, each presenting two different varieties of demanding visual search tasks, we found WMC to be unrelated to performance. Extreme groups of high- and low-WMC subjects showed statistically equivalent search slopes, and, within the continuous range of WMC scores in our sample, WMC showed near-zero correlations with search slopes and raw search RTs. In particular, we discovered span equivalence in feature-absence search, feature-conjunction search, spatial configuration search, and a command search task that required endogenous and strategic movement of attention through arrays. Given our large subject

samples, our varied measures of search, our steep and reliable search slopes, our reliable measures of WMC, and the sheer consistency of our null effects, we suggest that these null effects are real, important, and interesting (and that they meet Frick's, 1995, good effort criteria for accepting the null hypothesis). Any actual effect of WMC on visual search efficiency must be quite small, and it is certainly much smaller than those WMC-related effects seen in many other varieties of controlled, attentional performance (e.g., Conway et al., 2001; Kane et al., 2001; Kane & Engle, 2003; Long & Prat, 2002; Unsworth et al., 2004).

Knowing that individual differences in WMC do not translate into individual differences in prototypical visual search is important because it helps us to delimit the boundaries of the executive attention construct as it applies to variation in WMC (e.g., Engle & Kane, 2004; Engle, Kane, et al., 1999; Heitz et al., 2005; Kane et al., 2001, 2005; Kane & Engle, 2002). In our work, we have proposed that the executive attention processes that are correlated with WMC (and that drive the association between WMC and Gf) are those involved in (a) maintaining access to stimuli and goals outside of conscious focus, which is especially important—and measurable—in the face of interference from prior experiences or habit; (b) resolving response competition and restraining context-inappropriate responses; and (c) constraining conscious focus amid distraction.

We have characterized the attentional correlates of WMC this way for several reasons. First, we have found that WMC-related differences in memory-task performance were minimal or absent when sources of interference or response competition were re-moved (e.g., Bunting et al., 2004; Conway & Engle, 1994; see also Bunting, in press; May, Hasher, & Kane, 1999) and that high spans' normal superiority in resolving interference was abolished when their attention was divided during the task (Kane & Engle, 2000). Second, our view is consistent with viable theories of executive control and functional views of attention (e.g., Allport, 1980; Monsell, 1996; Neumann, 1987; Norman & Shallice, 1986), with commonly accepted characterizations of prefrontal cortex functions (e.g., Duncan, 1995; Fuster, 1999; Malmo, 1942; Miller & Cohen, 2001; Stuss, Shallice, Alexander, & Picton, 1995), and with the effects of aging and circadian arousal on attention control (e.g., Dempster, 1992; Hasher & Zacks, 1988; Hasher et al., 1999; West, 1996, 2001). Third, we have been influenced by this prior theoretical work to look for WMC-related effects in these particular attention control domains, so when we have found them it has naturally reinforced our views about the executive attention construct. As we reviewed in our introduction, these empirical findings include WMC-related individual differences in tasks requiring the restraint of habitual responses in favor of novel goals, such as in Stroop and antisaccade tasks, and in tasks requiring the constraint of auditory or (static) visual focus amid distractors, such as in dichotic listening, negative priming, flanker, and discontiguous-focus tasks.

However, our resulting view may be myopic: It is possible that we have defined executive attention too narrowly and that WMC is related to a host of attentional, or controlled, abilities and processes beyond the executive concerns that we and others have investigated, which have little or nothing to do with interference, distraction, and competition. We designed the present study to address this potential myopia. Nonetheless, we find no support for an expanded view of executive attention. In visual search tasks that present minimal competition between candidate responses or between habit and goal and that present minimal demands to

actively maintain or update goal-relevant information, but which are still difficult and thought to be sensitive to attentional limitations and to involve top-down attention control (Huang & Pashler, 2005; Wolfe et al., 2000), variation in WMC is of no measurable consequence. Thus, although there is obviously considerable generality to the executive attention construct, it does not seem to generalize to difficult attention tasks lacking the need to actively maintain goals to restrain prepotent responses or constrain attentional focus to particular stimuli or locations in space amid distractors.

Of course, there are other varieties of visual search that seem to require (or allow for) top-down control, and future work with these paradigms may reveal some sensitivity to variation in WMC. For example, it is perhaps not surprising that individual differences in WMC appear to predict performance in Sternberg-like memory search tasks, especially when stimuli are variably mapped onto target and distractor roles across trials (Conway & Engle, 1994; Oberauer, 2005). Thus, in contrast to the prototypical visual search tasks we have used in the present experiments, when a search task involves remembering and updating target information from trial to trial, high-WMC subjects seem to be advantaged relative to low-WMC subjects. On this basis, we predict WMC-related differences in a variably mapped visual search task that cues a new target stimulus or stimulus dimension (among only a few that repeat throughout the task) on each trial, not unlike the procedures explored by Schneider and Shiffrin (1977) and Shiffrin and Schneider (1977). WMC-related differences or other executive-related variation would be especially likely in this case if phonological rehearsal were effectively discouraged or prevented.

As another example, when the search target is a red horizontal bar amid many red vertical bars and few green horizontal bars, then color is less diagnostic of the target than is orientation. To the extent that people may exhibit top-down control over bottom-up influences (e.g., Wolfe, 1994), the bottom-up contribution of orientation should be amplified, the contribution of color should be reduced, and/or stimuli should be grouped according to the diagnostic feature. As evidence for this kind of modulation, when experimenters manipulate the proportions of nontarget features, subjects use this information to speed their search (e.g., Egeth et al., 1984; Zohary & Hochstein, 1989). We speculate that this top-down ability to amplify, dampen, or organize bottom-up influences could vary with WMC, but typical conjunction-search tasks (including the ones we used in the present experiments) prevent its expression by presenting equal numbers of nontarget types.

Yet another variety of top-down modulation that may occur during search is seen in cuing studies of noncontiguous attentional focus (e.g., Awh & Pashler, 2000; Juola, Bouwhuis, Cooper, & Warner, 1991). When discontiguous regions of space are cued as likely target locations, performance costs are seen with invalid cues even when the target appears between the cued locations. For example, Egly and Homa (1984) had subjects identify a letter at central fixation. At the same time, they presented another letter in 1 of 24 locations along three concentric rings around fixation, and subjects had to identify the letter's location (the entire array was masked). The ring on which the second letter would appear was cued (with 80% validity) before each trial with the word *close*, *medium*, or *distant*. As expected from "spotlight" or "zoom lens" theories, letters appearing outside the cued ring (outside the spot-light) on invalid trials were localized more poorly than were letters appearing along the cued ring. It is more interesting that letters appearing interior to the cued ring were also localized more poorly than were letters along the cued ring. These findings indicated that subjects flexibly configured attention discontiguously, focusing at fixation and on a ring beyond fixation, at the exclusion of an intermediary ring of space. As we reviewed earlier, Bleckley et al. (2003) tested high- and low-WMC subjects in the Egly–Homa task and found that only high spans demonstrated such flexible allocation, showing a cost when letters appeared on a ring interior to the cued ring. Low spans, in contrast, showed a benefit for any location on or interior to a cued ring, indicating a spotlight configuration. Together, these findings suggest that configuring attention flexibly is a controlled process linked to WMC variation, perhaps because it involves an active reduction of noise from nontarget items or locations (e.g., Dosher & Lu, 2000a, 2000b; Shiu & Pashler, 1994). Insofar as a more conventional search task may benefit from or require such flexible allocation, it should also produce WMC-related differences.

Indeed, such WMC-mediated search performance is provision-ally indicated in an ongoing line of experiments from our laboratory (Poole & Kane, 2005). In a spatial configuration search task using the same letter stimuli as in Experiment 3, subjects see a matrix of stimuli on each trial, and only some matrix locations are cued to be relevant before the search stimuli appear. Whether one, two, four, or eight discontiguous locations are cued on each trial, high-WMC subjects seem to identify targets in those locations significantly faster than do low-WMC subjects (with WMC ac-counting for about 10% of the variance in search RTs). Thus, when a static attentional focus must be constrained to include some locations and not others and when these locations must be constantly updated from trial to trial, higher WMC may be modestly associated with more efficient visual search.

By our inductive approach to theory development, then, the executive attention processes that correlate with individual differences in WMC—and that, we hypothesize, are largely responsible for the covariation between measures of WMC and general intellectual ability—are not universally involved in attention tasks. It is quite clear from prior work that when the movement of attention is exogenously triggered by environmental events, as in the prosaccade task (Kane et al., 2001; Roberts et al., 1994; Unsworth et al., 2004), WMC is unrelated to performance. Moreover, the present study goes further to show that some endogenous, or controlled, aspects of behavior are also independent of WMC. Although individual difference in WMC do predict the ability to restrain habitual responses (e.g., Kane et al., 2001) and constrain attentional focus against distractors (e.g., Conway et al., 2001), they do not predict the ability to move attention through displays to locate a consistent target stimulus presented amid distractors. At this time, then, the concept of executive attention seems best limited to those mental processes that (a) keep goals and goal-relevant stimuli accessible when they are outside of conscious focus (without the aid of practiced skills, e.g., phonological rehearsal) and in the face of significant interference from prior events, (b) stop un-wanted but strongly elicited behaviors in favor of novel responses, and (c) restrict conscious focus to target stimulus locations in the presence of task-irrelevant stimuli.

The Present Experiments and Alternative Views of WMC

Our finding that individual differences in WMC do not predict visual search efficiency in prototypical tasks rules out some alter-native explanations for the relation between WMC and other cognitive abilities. For example, equivalent search RTs and slopes for high- and low-WMC subjects suggest that low spans' poorer performance in other domains was not simply due to a lack of conscientiousness or motivation. In the present experiments, we engaged subjects in lengthy, monotonous tasks that required full seconds to respond accurately on each trial, yet low spans showed no performance deficit.

Cognitive speed theories cannot account for our results, either. Such theories are quite common in the intelligence and life span development literatures, and they hold that variation in WMC and in higher order intellectual abilities are caused by variation in the speed of more elementary cognitive processes (e.g., Fry & Hale, 1996; Jensen, 1987, 1998; Kail & Salthouse, 1994; Vernon, 1983). We have criticized such theories elsewhere (Conway, Kane, & Engle, 1999; see also Ackerman & Cianciolo, 2000), so we simply note here that a processing speed view must predict main effects of WMC in visual search RTs as well as WMC differences in RT search slopes (because as slowed processes are repeated across successive stimulus locations, the resulting slope will be greater). Of course, we found neither.

Finally, Oberauer (2005; Oberauer, Süß, Wilhelm, & Sander, in press) has proposed that WMC reflects the maintenance of bindings among activated mental representations and their spatiotemporal contexts in a WM region of direct access (which is analogous to Cowan's, 1999, 2001 capacity-limited focus of attention). By this view of WMC, people differ in the number and/or quality of independent bindings that can be established and maintained in an accessible state, and thus variation in WMC represents primarily variation in a "limited capacity for relational integration" (Oberauer et al., in press, p. 7). This view is relevant to the present work because Oberauer et al. suggested that our previous findings of WMC-related variation in Stroop and antisaccade tasks might have resulted from individual differences in binding representations of task-relevant stimulus properties to representations of required responses. That is, antisaccade and Stroop tasks ask subjects to respond to stimuli in nonautomatic ways, and thus they provide low stimulus–response (S-R) compatibility. Low spans, by this view, may therefore perform worse in such tasks because they have more difficulty

establishing or maintaining arbitrary response mappings than do high spans. Although the visual search tasks we used here did not provide conflict with habitual responses, the response mappings were arbitrary and thus low in S-R compatibility. Subjects either pressed one key to indicate the presence of a target and another to indicate its absence (Experiments 1 and 2) or pressed one key to indicate an *F* target was present and another to indicate a backward *F* target was present (Experiment 3). Low spans appeared to have no more difficulty binding these stimulus dimensions to arbitrary response mappings than did high spans.

Before leaving a consideration of alternative views of WMC, we should explicitly note that our results do not seem to us to discriminate our executive attention view of WMC from the inhibitory control view of Hasher, Zacks, and their colleagues (e.g., Hasher & Zacks, 1988; Hasher et al., 1999; Lustig et al., 2001). In short, the inhibition view holds that variation in WMC is driven by a more primitive variation in attentional inhibition. We argue, in subtle contrast, that a single attention control capability causes variation both in active inhibition (or other mechanisms of blocking, restraint, and constraint) and in active maintenance. With respect to search, one could conceivably argue that because visual search tasks present nontarget distractors, they might measure inhibitory control to some degree (e.g., Klein, 1988; Zacks & Hasher, 1994). If so, then our findings would be problematic for an inhibitory view of WMC. However, we do not think that the visual search literature makes a strong case that active inhibitory control is particularly important to the search process, especially when the locations of the distractors are not known in advance.

Do Dual-Task Experiments Suggest a Role for WMC in Visual Search?

Our correlational data suggest that WMC is unnecessary for effective visual search performance, even in very demanding search tasks. What does the vast experimental literature on visual search seem to say about a possible role for memory? On one hand, there is a controversy over whether visual search "has a memory" or not. Subjects do seem to remember targets that they have found (e.g., Gibson, Li, Skow, Brown, & Cooke, 2000; Horowitz & Wolfe, 2001) and to find targets more quickly when some aspects of stimulus displays are repeated (e.g., Chun & Jiang, 1998; Jordan & Rabbitt, 1977; Rabbitt, Cumming, & Vyas, 1979). However, it is less clear whether subjects remember not to revisit distractor locations they have already searched. Some studies have indicated such inhibition of return to searched locations (e.g., Klein, 1988; Muller & von Muhlenen, 2000), but others have found either no inhibition of return or inhibition of only the last few locations that were searched (e.g., Horowitz & Wolfe, 1998; Snyder & King-stone, 2000; Wolfe & Pokorny, 1990). In his recent review of this literature, Wolfe (2003) argued that observers retain only a very limited memory for the progress of their searches and do not search stimuli or locations without replacement.

On the other hand, biased-competition theories of visual search (e.g., Desimone, 1996; Desimone & Duncan, 1995; Duncan, 1998) strongly suggest that another kind of memory process, more strongly related to notions of WMC, should be important to successful search—namely, active maintenance of a target template. As behavioral support for this idea, Downing (2000) found that when subjects had to actively maintain a novel target in memory over a delay, such as the picture of a face, their attention was automatically drawn toward subsequent appearances of that face, even when these subsequent faces were presented very quickly as task-irrelevant stimuli. In contrast, when a face was seen without the requirement to remember it, attention was actually drawn away from subsequent appearances of that face. Thus, active maintenance of visual representations seems to affect the guidance of visual attention.

Indeed, a strong prediction of these biased-competition views— that interfering with active memory should impair search—has also gained empirical support. Although Woodman, Vogel, and Luck (2001) originally reported that visual search performance was unaffected when subjects' visual WMC was loaded, subsequent research has shown that visuospatial memory loads do disrupt search. Woodman et al. embedded an inefficient-search task within the delay period of a matching-to-sample task that presented subjects with a sample of four stimuli (colored squares or Landolt *C*s of different orientations) that either matched a subsequent test probe of four items or differed from the probe by just one item. In three experiments, RT increased under visual memory load, but search slopes were unaffected. Follow-up studies, in contrast, used memory-load stimuli that required retention of spatial relations. Woodman and Luck (2004) presented sequences of two dots as the memory

sample and a single probe dot that either matched one location from the sample or did not. Oh and Kim (2004) presented an array of four squares as the sample and a single probe square that could match (or not) one location from the sample. Both studies with spatial memory demands found that the load task increased search slopes by 20–30 ms relative to no-load trials.

One might try to reconcile the memory-load experimental findings with our individual-differences findings by noting that we measured WMC using dual tasks that are thought to engage executive processes, whereas the dual-task studies loaded WMC using storage-only tasks of spatial memory. However, we have previously found that WMC span tasks using verbal stimuli correlate reasonably well with storage-only spatial tasks, and a growing literature suggests some domain-general executive involvement in the storage of visuospatial information (see Kane et al., 2004, for a review). Moreover, verbal memory tasks that require manipulation of encoded information, and so likely draw on executive processes to some extent, also appear to impair visual search when they are presented as secondary tasks. Han and Kim (2004) presented the Woodman et al. (2001) search task during the delay period of verbal memory tasks in which subjects either (a) subtracted by threes from a different three-digit number on each trial (Experiment 1A) or (b) reordered a four-letter string into alphabetical order (Experiment 2A). At the end of each trial, after completing the search task, subjects reported the final calculation (Experiment 1A) or the alphabetized letter sequence (Experiment 2A). Both of these nonspatial, executive memory loads increased search slopes by 50–70 ms compared with analogous verbal storage-only tasks of memorizing digit strings (Experiment 1B) or letter strings (Experiment 2B).

Although none of these memory-load studies used the exact search tasks we did, they are reasonably similar to our feature-absent and spatial configuration tasks, so it is unclear what to make of the discrepancies between the experimental and correlational findings. It thus remains a mystery why dual-task studies suggest WMC to be important to search efficiency, whereas the individual-differences studies we report suggest WMC to be largely irrelevant to prototypical laboratory tests of inefficient search. Perhaps future work that combines experimental manipulations of WMC with naturally occurring individual differences in WMC will help to unravel the mystery.

Notes:

1 Our use of the term *executive attention* in this article (and, e.g., Engle & Kane, 2004; Kane & Engle, 2002, 2003), rather than *controlled attention* (e.g., Engle, Kane, & Tuholski, 1999; Kane, Bleckley, Conway, & Engle, 2001), reflects less a demarcated change in thinking than a wish to emphasize a debt and family resemblance to other theories of executive function, executive control, and executive attention (e.g., Baddeley & Logie, 1999; Norman & Shallice, 1986; O'Reilly, Braver, & Cohen, 1999; Posner & DiGirolamo, 1998).

2 Wolfe (1994) noted that his model could, in principle, be implemented as a parallel processing model, in which a limited attentional resource is divided among potential target locations commensurate with activation levels at those locations. We therefore do not engage the serial versus parallel debate here (nor can our data adjudicate it).

3 Although Figures 5A and 5B appear to indicate an overall RT difference between high- and low-WMC subjects in both target-present and target-absent trials, when we combined these trial types into a supplemental analysis to increase our power, the main effect of WM span was still nonsignificant, $F(1, 138) = 2.19$, $MSE = 90{,}143.70$, $p = .14$, and all interactions involving WM span yielded Fs < 1.'

4 We thank Eric Ruthruff for making this suggestion.

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